339

SMITHSONIAN

MISCELLANEOUS COLLECTIONS.

VOL. XX.

"EVERY MAN IS A VALUABLE MEMBER OF SOCIETY WHO BY HIS OBSERVATIONS, RESEARCHES, AND EXPERIMENTS PROCURES KNOWLEDGE FOR MEN."—SMITHSON.

WASHINGTON:
PUBLISHED BY THE SMITHSONIAN INSTITUTION.
1881.

VOL. XX.

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WASHINGTON, D. C.

ADVERTISEMENT.

The present series, entitled "Smithsonian Miscellaneous Collections," is intended to embrace all the publications issued directly by the Smithsonian Institution in octavo form; those in quarto constituting the "Smithsonian Contributions to Knowledge." The quarto series includes memoirs embracing the records of extended original investigations and researches resulting in what are believed to be new truths, and constituting positive additions to the sum of human knowledge. The octavo series is designed to contain reports on the present state of our knowledge of particular branches of science; instructions for collecting and digesting facts and materials for research; list and synopses of species of the organic and inorganic world; museum catalogues; reports of explorations; aids to bibliographical investigations, etc., generally prepared at the express request of the Institution, and at its expense.

The position of a work in one or the other of the two series will sometimes depend upon whether the required illustrations can be presented more conveniently in the quarto or the octavo form.

In the Smithsonian Contributions to Knowledge, as well as in the present series, each article is separately paged and indexed, and the actual date of its publication is that given on its special title page, and not that of the volume in which it is placed. In many cases, works have been published, and largely distributed, years before their combination into volumes.

While due care is taken on the part of the Smithsonian Institution to insure a proper standard of excellence in its publications, it will be readily understood that it cannot hold itself responsible for the facts and conclusions of the authors, as it is impossible in most cases to vertify their statements.

SPENCER F. BAIRD,
Secretary Smithsonian Institution.

CONTENTS.

RULES

FOR THE

PUBLICATION OF THE BULLETIN.

I.

The general rule will be maintained of publishing in the Bulletin only titles and abstracts of papers, and the latter only when presented to the Secretary by the author within three days after the evening of the reading.

The President's annual Address will be published in full.

II.

When directed by the General Committee, any communication may be published in full in an Appendix to each volume of the Bulletin.

III.

Of the remarks made consequent on the reading of any communication, only such will be published as are sent in abstract in writing to the Secretary within three days after the evening of their delivery.

IV.

Communications that have been published elsewhere so as to be generally accessible will appear in the Bulletin by title only, but, if possible with a reference to the place of publication.

V.

In reference to communications made to the Society previous to this date:

When the original paper has been filed with the Secretary in manuscript in full, it will be published in abstract only, unless otherwise ordered by the General Committee.

WASHINGTON, January 17, 1874.

[1.]

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON, D. C.

Vol. I.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON.
1874.

ANNIVERSARY ADDRESS

OF THE

PRESIDENT OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON,

PROF. JOSEPH HENRY.

(Delivered November 18, 1871.)

GENTLEMEN:

I have been requested to make some remarks on the character and object of this Society which may serve to introduce it to the world through the pages of a Bulletin of its proceedings or the public journals of the day, and in compliance with this request I beg leave to submit the following.

This Society was formed by the call for a meeting of a number of gentlemen impressed with the importance of an association of a strictly scientific character in the city of Washington. At the meeting which resulted from this call a name and a constitution were adopted for the Society, and without delay, in a series of subsequent meetings, the objects of the association were prosecuted with such marked success as to fully realize the anticipations which had been entertained with regard to the enterprise. This is manifest from the number, character, and variety of the communications presented and discussed.

In regard to the name which has been chosen, "THE PHILOSOPHICAL SOCIETY OF WASHINGTON," it is proper to remark that it was adopted not without considerable

deliberation. The term "Philosophical" was chosen not to denote, as it generally does, in the present day, the unbounded field of speculative thought, which embraces the possible as well as the actual of existence, but to be used in its restricted sense to indicate those branches of knowledge that relate to the positive facts and laws of the physical and moral universe. The second term, "Washington," was selected to denote the fact that the Society is a *local* establishment; that it arrogates to itself nothing on account of its position at the national capital; makes no claim to any connection with the government, nor to being, in any respect, a special representative of the science of the country.

The importance of such a society must be evident to all who are acquainted with the history of science. It is mainly through the influence exerted and the assistance rendered by such associations that science is advanced and its results given to the world. Man is a sympathetic being, and no incentive to mental exertion is more powerful than that which springs from a desire for the approbation of his fellow men: besides this, frequent interchange of ideas and appreciative encouragement are almost essential to the successful prosecution of labors requiring profound thought and continued mental exertion. Hence, it is important that those engaged in similar pursuits should have opportunities for frequent meetings at stated periods; this is more particularly the case with the cultivators of abstract science, who find but comparatively few fully capable of appreciating the value of their labors, even in a community how much soever enlightened it may be on general subjects. The students of history, of literature, of politics, and of art find everywhere men who can enter in some degree into their pursuits, and who can appreciate their merits and derive

pleasure from their writings or conversation; while the mathematician, the astronomer, the physicist, the chemist, the biologist, and the student of descriptive natural history meet with few, comparatively, who can sympathize with them in their pursuits, or who have a sufficient knowledge of their particular subjects to be able to award them that intelligent appreciation and encouragement essential to their sustained and laborious efforts. To them, the world consists of a few individuals, to whom they are to look for that critical judgment of their merits which is to be finally adopted by the general public, and with these it is of the first importance that they should have more frequent intercourse than that which arises from casual meetings.

Furthermore, a society of this kind becomes a means of instruction to all its members, the knowledge of each becoming, as it were, the knowledge of the whole. Again, there is a common bond of union between all branches of science, since they all relate to the existence and laws of the same universe in which the more we extend our knowledge the more we find of "unity in the midst of infinite diversity." This connection is obvious in the relations of astronomy, mathematics, and physics, as well as in those of geology, chemistry, and biology, which are so closely related in many cases as to be separable only by conventional limits. In a society, therefore, like the one in question, embracing in its objects, as it does, all branches of science, each investigator may find others cultivating fields separated from his own by insensible degrees, from whom he can have not only full sympathy and adequate appreciation, but also, in many instances, important suggestions and essential aid.

The governing body of such a society, in order that the

organization may produce the desired effect, must be largely composed of men who, by education and experience in the processes of investigation, are justly entitled to the appellation of "scientific," and who, from their positive contributions to the science of the day, are acknowledged by the scientific world as worthy of this distinction. It is true, useful societies are formed for the self-improvement of their members by the production of essays on various subjects, or by cultivation of branches of natural history requiring no previous special training: the city of Washington, however, needs something of a higher order, namely, a society for the *advancement* of science, since in no other city in the Union are there so many men, in proportion to the population, connected with scientific pursuits, or so many facilities for scientific investigation.

The Philosophical Society of Washington, though of a local and unostentatious character, may, if true to itself and its mission, accomplish much towards increasing the reputation of the country and influencing public opinion with regard to questions of a scientific character. However wide the diffusion of general knowledge, public opinion in regard to scientific questions must eventually be determined by the authority of societies, journals, and individuals, of established scientific reputation. It is therefore of the first importance that the operations of this Society be conducted with great care, and that nothing be given to the world under its sanction which is not based upon thorough investigation or established scientific principles. We should be warned by the fate of a society established in this city some thirty years ago, which, although it included among its members a few men of true science, was under the control principally of amateurs and politicians, and therefore

was unfit to discharge the duty which it claimed as one of its functions, to decide questions of a strictly scientific character. It should have been borne in mind by this association that votes on questions in science should be *weighed*, not counted! Had the proposition of the motion of the earth been decided in the days of Galileo by the popular voice, this philosopher and his friends would have been vastly in the minority. The society to which I allude, after achieving an unenviable notoriety, by assuming to be the arbiter of the science of the country, gradually sunk into oblivion, from which its memory should not be recalled except as a warning to those who would adventure in the same line.

It is an essential feature of a scientific society that every communication presented to it should be subject to free critical discussion. Such discussion not only enlivens the proceedings, but is, generally, instructive, frequently eliciting facts which, though insignificant when isolated, when brought together mutually illustrate each other, and lead, ultimately, to important conclusions. The extent to which discussions may be allowed evidently depends on the candor and temper of those who engage in them. Among the things to be avoided are, merely verbal criticism, undue harshness on the one hand, and unmerited praise on the other, regard being had to truth rather than to victory or mutual adulation. There is nothing, perhaps, which marks more distinctly one of the characteristics of a true scientist than the manner in which he receives and appropriates to his use the critical remarks that may be made upon his communications. He can, in many cases, at least, derive from them the indication that he has failed to present on some points a clear statement of his investigations;

or that in some other points his conclusions are not fully sustained by the premises. Unfortunately, it frequently happens that persons of a sensitive disposition are apt to consider criticism of the kind we have mentioned as personal attacks, and that it is as offensive to doubt the accuracy of their experiments or conclusions as it is to doubt their word. It should, however, be recollected that the most gifted are liable to err, and that these criticisms are *prior* to publication, and, therefore, of value to the permanent reputation of both the individual and the society.

Another important matter in regard to such a society is the publication of its proceedings. If its object were merely the intellectual and moral improvement of its members, it might dispense entirely with any publication whatever—even with the announcement of its existence. If, however, it aspires to the more important office of *advancing* science, or of enlarging the bounds of thought and assisting to diffuse a knowledge of new truths, it should then publish, if not quarto volumes of transactions, at least a bulletin of its proceedings. This publication should present an exposition of the organization of the society, its constitution and by-laws, give a list of the members, a synopsis of the contents of all communications submitted for consideration, and an account of important facts which may be elicited during discussions or recalled to memory at the moment by association of ideas.

Such a bulletin will enable the members of the society to publish without delay, through a proper channel, a synopsis of their investigations, and, also, minor facts and inferences not considered, in themselves, of sufficient importance to form a communication to a scientific journal or to occupy a place in philosophical transactions. Such

facts are, nevertheless, frequently found to be valuable contributions to the general stock of knowledge. Were it possessed of the requisite funds, the society might establish a higher reputation by the publication of independent transactions. Inasmuch, however, as this is not the case, the next best plan should be adopted, namely, that of publishing papers in full through other channels, such, for instance, as the Smithsonian Institution, the reports of government bureaus, and scientific journals. In such cases, the bulletin should contain references as to where the articles in full are to appear, and in this respect it would do good service in assisting to make more generally known the valuable contributions to science which are diffused through voluminous executive and congressional documents not readily accessible to the scientific world.

The editing of the bulletin should be under the direction of the secretaries and a committee appointed for the purpose, and a number should be issued as often as material of the proper character and of sufficient quantity is accumulated. It should be distributed to the principal learned societies of this and other countries, and may also be presented to leading journals in this and other cities. Without at least such a publication, the society cannot have a recognized existence.

I have stated that there is no city in the United States, in proportion to the number of its inhabitants, where there are so many men of education actively engaged in pursuits connected with science as in Washington. In illustration of this remark I may refer to those who are engaged in the Coast Survey, the Office of Weights and Measures, the National Observatory, the Nautical Almanac, Patent Office, Engineer Department, Hydrographic Office, Ordnance De-

partment, Medical Departments of the Army and Navy, Lighthouse Board, Signal Corps, Agricultural Department, Bureau of Statistics, Census Office, Bureaus of Navigation and Steam Engineering, the Smithsonian Institution, etc. etc. In addition to this, no city in the Union possesses more ample facilities, in the way of books and implements, for the prosecution of scientific research. The library of Congress, enriched by the Smithsonian Deposit with the transactions of all the principal learned societies of the world, is almost unrivalled in scientific works. If to this extensive collection we add the special libraries of the Patent Office, the Agricultural Department, the Coast Survey, the National Observatory, and of the Surgeon-General's Office, we have a collection of modern books on science, accessible to the members of the society, scarcely surpassed by the collections of the most favored cities of the old world. Nor are the articles of apparatus necessary for any line of investigation beyond the reach of any member of the Society who may possess the knowledge and skill requisite to their proper use. There is great liberality on the part of the heads of departments in the way of furnishing apparatus that may in any degree facilitate the special investigations under their direction.

Among those connected with the various organizations just mentioned, a considerable number are engaged in *original* investigations, the results of which are of interest to the scientific world, and which will be facilitated and improved by the discussions of this Society. Furthermore, in the daily operations of the different establishments, facts of scientific importance are continually becoming evident which would be lost if not preserved in the records of the Society. It is not, however, alone to facili-

tate operations now going on, or to preserve facts that may have been casually discovered, but, also, to suggest new investigations and to encourage others to enter the field of research who have not yet essayed their hand in this direction. In the great domain of science, there is abundant room for an infinite number of laborers of different grades of attainment and original powers of mind. A series of careful observations made with proper instruments, with regularity and precision, which requires little more than the exercise of the senses and a conscientious regard for truth, is frequently a valuable contribution to science. A series of analyses in which prescribed formulas are observed, and in the application of which no more talent is required than that which is possessed by the majority of persons of ordinary ability and education, may give results of scientific value. For the production of results of the kind mentioned, and those which are effected by the scientist who is capable of detecting hitherto undiscovered facts and developing new laws, there is room for all grades of talent and of powers of original investigation. It is astonishing how much may be done by the association of minds determined on a common pursuit; how much, under such conditions as exist in the city of Washington, may be effected in the way of directing attention to special lines of investigation, in suggesting questions to be asked of nature, and in pointing out the ready means by which the answers may be elicited, by arousing into activity talents which, without such stimulus and suggestion, would ever remain dormant.

The bane of many societies is the time consumed in details of business and in the discussion of non-essential points relative to their government. Happily, the organ-

ization adopted by this Society obviates this evil and secures the devotion of almost every evening exclusively to its legitimate purposes. For the government of men whose object is the advance of *truth*, but few rules are necessary, and these, unlike the laws of the Medes and Persians—expressed in inexorable codes—must consist of simple principles, readily adaptable to all contingencies.

In conclusion, I would say that with so many facilities as exist in the city of Washington for the pursuit of science, this Society would be derelict of duty did it fail to materially aid, through communion of thought and concert of action, the advancement of the great cause of human improvement. I am happy, however, in cherishing the opinion that the success of "The Philosophical Society of Washington" is scarcely any longer problematical, and in this I am sustained by the record of its transactions.

CONSTITUTION

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE III. There shall be a General Committee, consisting of the officers of the Society, and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee, shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

ARTICLE VI. This Constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

The General Committee also reported the Standing Rules for the government of the Society, which had been enacted under the fifth article of the Constitution.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday, the place of meeting to be designated by the General Committee.

2. The Annual Meeting for the election of officers shall be the first stated meeting in the month of November. When necessary, Special Meetings may be called by the President.

3. Notices of the time and place of meetings shall be sent to each member by one of the Secretaries.

4. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

5. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

6. New members shall be elected by the General Committee, after having been proposed in writing by at least three members of the Society.

7. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers.

The Committee further reported for the information of the members of the Society, the Standing Rules which they had enacted for the government of the General Committee, viz.:—

STANDING RULES

OF THE

GENERAL COMMITTEE

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

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1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees, one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half past seven o'clock on the evening of each stated meeting, and by adjournment at other times.

6. For all purposes, except for the amendment of the Standing Rules of the Committee and of the Society, and the election of members, six members of the Committee shall constitute a quorum.

BULLETIN

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

This Society had its origin in the following initiatory letter:—

Prof. Joseph Henry, LL.D.

The undersigned respectfully request you to preside at a meeting which they propose to hold for the purpose of forming a society, having for its object the free exchange of views on scientific subjects, and the promotion of scientific inquiry among its members.

M. C. Meigs,
Benjamin Peirce,
Theo. Gill,
Peter Parker,
F. B. Meek,
T. R. Peale,
Wm. B. Taylor,
Chas. A. Schott,
E. B. Elliott,
F. V. Hayden,
J. E Hilgard,
J. H. Lane,
S. F. Baird,
Walter L. Nicholson,
Wm. H Dall,
B. Franklin Greene,
S. V. Benét,
Horace Capron,
Thomas Antisell,
J. J. Woodward,
J. S. Billings,
J. K. Barnes,
C. H. Crane,
George A. Otis,
Albert J. Myer,
A. A. Humphreys,
Asaph Hall,
Simon Newcomb,
Wm. Harkness,
B. F. Craig,
J. H. C. Coffin,
Thornton A. Jenkins,
George H. Elliot,
W. T. Sherman,

GEORGE C. SCHAEFFER,
THOS. LINCOLN CASEY,
JNO. G. PARKE,
B. F. SANDS,
A. B. DYER,
J. B. WHEELER,
A. B. EATON,
ELISHA FOOTE,
SALMON P. CHASE.

1ST MEETING. MARCH 13, 1871.

Prof. JOSEPH HENRY in the Chair.

In response to this call, a meeting of the subscribers thereto was convened and held at the Smithsonian Institution, in the Regent's room, on Monday, March 13, 1871. The outline of a Constitution was adopted, and under it the following gentlemen, who collectively should constitute a GENERAL COMMITTEE for the transaction of the business of the Society, were elected officers:—

PRESIDENT.

JOSEPH HENRY.

VICE-PRESIDENTS.

M. C. MEIGS,
J. E. HILGARD,
HORACE CAPRON,
WM. B. TAYLOR.

TREASURER.

PETER PARKER.

SECRETARIES.

B. F. CRAIG,
THEODORE GILL.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

THOMAS ANTISELL,
J. H. C. COFFIN,
S. NEWCOMB,
S. F. BAIRD,
E. B. ELLIOTT,
W. T. SHERMAN,
T. L. CASEY,
T. A. JENKINS,
J. J. WOODWARD.

The Constitution was then referred to the General Committee for verbal expression; and the Committee was also empowered to propose Rules and By-Laws for the government of the Society.

2D MEETING. MARCH 18, 1871.

The President in the Chair.

Professor S. F. BAIRD communicated to the Society, on behalf of the author, a copy of a memoir entitled—

OFFICIAL REPORT OF THE YELLOWSTONE EXPEDITION OF 1870, BY LIEUT. G. C. DOANE, 2D U. S. CAVALRY.

(This Report will be found published in full as Official Document, Senate, No. 51, 41st Congress, 3d Session.)

3D MEETING. APRIL 1, 1871.

The President in the Chair.

The General Committee reported to the Society the "Constitution," which was adopted.

(For the Constitution and Standing Rules see pp. 14, 15, 16.)

The President announced that the following gentlemen had been appointed on the Sub-Committees, viz.:—

SUB-COMMITTEES.

ON PAPERS AND ESSAYS:

Chairman, J. J. WOODWARD;
S. L. CASEY, J. E. HILGARD.

ON PUBLICATION:

Chairman, S. F. BAIRD;
B. F. CRAIG, THEODORE GILL.

The transaction of business being concluded, General M. C. MEIGS laid before the Society a map of the head waters of the Yellowstone and Lewis rivers.

Mr. J. E. HILGARD read a paper entitled—

ON THE DISTANCE TRAVERSED BY APPROACHING THE NORTH POLE ON A LOXODROMIC CURVE.

Mr. J. E. HILGARD stated that having had occasion to determine the geographical centre of the United States, or the centre of gravity of the surface of the country, he had ascertained it to be near the Black Hills of Wyoming Territory

Professor JOSEPH HENRY made an oral communication

ON PHENOMENA OF SOUND AND EXPERIMENTS WITH TUNING FORKS:

illustrating his statements by means of a set of resounding cavities.

Dr. WM. THOMSON of Philadelphia communicated a memoir entitled—

ON A NEW METHOD FOR DETECTING AND MEASURING THE OPTICAL DEFECTS OF THE EYE.

(*The substance of this communication is published under the title, "An Additional Test for the Diagnosis and Correction of the Optical Defects of the Eye," in The American Journal of the Medical Sciences, N. S., vol. lix., pp.* 76–80.)

4TH MEETING. APRIL 15, 1871.

The President in the Chair.

Mr. J. E. HILGARD exhibited a chronograph of the kind devised by Hippe of Neuchatel, and explained its construction.

Gen. A. B. EATON made a communication

ON THE PRESERVATION OF FOODS,

and exhibited specimens of preserved meats.

Col. T. L. CASEY communicated a Report by Capt. Chas. W. Raymond, U. S. Engineers,

ON THE RESULTS OF TRAVELS IN ALASKA AND THE DETERMINATION OF THE POSITION OF FORT YUKON.

(*This Report is published as Executive Document No.* 12, *Senate,* 42*d Congress,* 1*st Session.*)

5TH MEETING. APRIL 29, 1871.

The President in the Chair.

Admiral B. F. SANDS communicated, in the name of Professor ASAPH HALL, an abstract of a paper

ON THE ELEMENTS OF THE COMET I, 1871.

(ABSTRACT.)

This comet was discovered by Dr. Winnecke at Carlsruhe, April 7th, and was also independently discovered by Mr. Lewis Swift at Marathon, New York, on April 15th, 1871. Notice of its discovery was received at the Naval Observatory April 19th, and it was observed there on the 20th, 23d, and 24th, cloudy weather preventing further observations. Although the observations are not well situated to give a good determination of the orbit, the following elements have been computed :—

PERIHELION PASSAGE, 1871, June 10, 496, Washington mean time.

Longitude of Perihelion,	140°	49′	13″	Apparent Equinox April 22.
Longitude Ascending Node,	279	3	31	
Inclination of Orbit Plane,	87	42	23	

Logarithm Perihelion Distance, 9.82206.

Motion Direct.

Computing with these elements the Altona observation of April 9th, the following differences are found—

$$d\lambda = -0'.8, \text{ and } d\beta = +0'.1:$$

from which it appears that these elements must be a tolerable approximation to the truth.

When first discovered the comet was about 95 degrees in true anomaly from its perihelion, and is therefore approaching the Sun, and the computations show that it is slowly approaching the Earth. Its motion, however, in heliocentric longitude is so small that it will be apparently very near the Sun. Were it not for this it might become visible to the naked eye after the next full moon. Its distance from the Earth is about 1.8 of the Sun's mean distance, and as it is easily observed in the telescope it is in reality a bright comet.

Mr. J. E. HILGARD exhibited a chronoscope devised by Hippe, of Neuchatel, and explained its application in the determination of the rate of transmission of nerve-power.

By request, Major KING made a verbal report

ON THE CONSTRUCTION OF THE BRIDGE ACROSS EAST RIVER BETWEEN BROOKLYN AND NEW YORK.

6TH MEETING. **MAY 13, 1871.**

The President in the Chair.

Prof. S. F. BAIRD communicated a paper by Dr. H. B. BUTCHER

ON TWO IMMENSE METEORITES AT CONCEPTION AND SAN GREGORIO, MEXICO.

(Concerning the subject of this communication, see "The precise geographical position of the large mass of Meteoric Iron in Northern Mexico [etc.] by J. LAURENCE SMITH; The American Journal of Science, 3d series, vol. ii., pp. 335–338.)

Dr. J. J. WOODWARD made a communication

ON THE ALLEGED HERMAPHRODITE DESCRIBED BY DRS. ACCLY, BLACKMAN, AND JACKSON.

(This communication is published under the title, "Remarks on a Supposed Case of Hermaphroditism," in The American Journal of the Medical Sciences, N. S., vol. lxii., pp. 123–125, *July*, 1871.)

(ABSTRACT.)

Dr. Woodward exhibited a wet preparation of the generative organs in a case of supposed hermaphroditism, and a plaster cast of the same. The specimen was originally described in the American Journal of the Medical Sciences, July, 1853, and has subsequently been quoted in works on medical jurisprudence as one of hermaphroditism. It was recently presented to the Army Medical Museum, and being well preserved in alcohol, was subjected to further dissection and a careful microscopical examination. The parts supposed to be ovaries were simply little masses of adipose tissue, and the case was in fact merely one of undescended testicle in an otherwise well-developed male.

Dr. THEODORE GILL read a paper

ON THE CHARACTERISTICS AND ZOOLOGICAL RELATIONS OF MAN.

(ABSTRACT.)

Prof. Gill adverted to the various beliefs respecting the origin of man, contrasted those of Lord Monboddo and Darwin, and successively enumerated those characters which man shared in common with animals generally, and in addition thereto, with Vertebrates, with all Mammals, with the placental mammals, with

the "Educable" division of the latter, and with those constituting the order Primates and the suborder Anthropoidea. After the elimination of those forms whose structure was thus shown to be successively removed from his, he contrasted man in a final term with the apes, and expressed his conviction that there exist no structural (morphological) characters which would be admitted to possess more than family value, were we able to divest ourselves of personal and psychological prejudices, and asserted that such was the view of the majority of the most approved students of the mammals. Man's relations might be exhibited in a quasi-genealogical table by a stem from the same common branch as the higher apes, and if the doctrine of evolution is accepted at all, such a table would express for the believer therein the fact of a derivation of man and the highest apes from the same common and specialized stock.

7TH MEETING. MAY 27, 1871.

The President in the Chair.

Dr. T. ANTISELL exhibited, on the part of the Department of Agriculture, a vial of dust charged with organic matter from Bitliz, Armenia.

Mr. W. H. DALL presented a paper

ON THE RELATIVE VALUE OF ALASKA TO THE UNITED STATES, AS COMPARED WITH THAT OF OTHER TERRITORIAL ACQUISITIONS.

(This article was published in full under the title "Is Alaska a Paying Investment?" in Harpers' New Monthly Magazine, vol. xliv., pp. 252–257, January, 1872.)

(ABSTRACT.)

After submitting estimates of the cost and value of Texas, Florida, and New Mexico (including Arizona), and showing that the direct taxes paid into the Treasury of the United States cannot be taken as an index of the value to the community at large of any given region, Mr. Dall gave the following statement of items as the data on which to found an estimate of the present value of the productions of Alaska.

Direct taxes as established by law :—

Annual rental of seal fishery	$55,000 00
Tax on seal skins, 50,000 allowed to be taken by present regulations	100,000 00
Bonus, 62½ cents each, on such skins	31,250 00
Bonus on oil 55 cents per gallon, of which the yield is estimated at one gallon for each seal killed . . .	27,500 00
Supplies and schools to be furnished to natives of seal islands	2,000 00

Productions :—

Value of seal skins above taxes	318,750 00
Furs from Yukon district annually	75,000 00
Other continental furs	10,000 00
Fish and furs of Sitkan district according to Maj. Tidball's Report	51,000 00
Annual yield of sea otter trade, estimated at one-third the annual yield of the last twenty years . . .	65,000 00
Walrus ivory and oil (1868)	7,500 00
Salt codfish (10,612,000 lbs. in 1870)	754,840 00
Cod-liver oil (10,000 gallons in 1866)	10,000 00
Whale oil and bone from Alaskan waters, estimated at one-third the whole Behring Sea catch annually, viz., 466,666 lbs. bone and 1,179,000 gallons oil . .	869,499 60
Ice trade	28,000 00
Spars and timber	2,000 00
Total annual production	$2,407,339 60

From this are to be deducted the annual expenses of the Territory and the cost of production of the various articles, leaving as a net profit over all expenses of every description, past and present, the sum of $674,201 30 annually. This is equivalent to a net profit of eight per cent.; while by similar estimates Florida pays four per cent., Texas twenty per cent., and New Mexico less than nothing, annually.

The total cost of Alaska up to Jan. 1, 1871, is $8,873,370 25, and the present annual expenses are $529,468 50.

Dr. THEODORE HILGARD, of St. Louis, made a verbal communication

ON THE NUMBER OF THE CEPHALIC VERTEBRÆ.

(This communication is published in full in the Proceedings of the American Society for the Advancement of Science, Twentieth Meeting.)

8TH MEETING. JUNE 10, 1871.

The President in the Chair.

Mr. W. B. TAYLOR presented a memoir

ON THE NATURE AND ORIGIN OF FORCE.

(This communication is published in full under the title, "Thoughts on the Nature and Origin of Force," in the Annual Report of the Board of Regents of the Smithsonian Institution, for 1870, *pp.* 241–257.)

(ABSTRACT.)

The "Conservation of Force" is not an axiom, as it seems natural to regard it, but a corollary—dependent on the coexistence in all matter of two opposing tendencies, attraction and repulsion. Were any form of matter absolutely incompressible (and hence inelastic), there would be in every case of the collision of such matter, a simple destruction of *vis viva.*

Motion is not the only exhibition of force—as we have a large array of static forces—nor the measure of force, since it is not proportional thereto, but follows the law of the square root of the power originating it. Contrary, therefore, to a not unusual generalization, motion is not persistent, but may be destroyed, resulting in static force, and it may be created or produced from static force. The phenomena of so-called "latent heat" may be cited as an example.

If we accept the nebular hypothesis of the cosmogony, all force was originally static; and observation shows us that it is on the whole ever becoming more and more kinetic, ever more and more equably diffused; the tendency being steadily to what Professor Sir William Thomson has designated the "Dissipation of Energy." The sum of the static or potential, and of the kinetic forms of energy, is of course forever constant, or unalterable.

The so-called "vital forces" have been shown in recent times to be but a portion of the preëxisting store of purely mechanical energy. Not only organic nutrition, growth, and movement, but the more subtle processes of thought and emotion are maintained from without, and are dependent on material changes and the transference of molecular motion. All animal power (like all mechanical power employed by man) is derived ultimately from the vegetable storehouse of chemical energy; and the store of static vegetable force is derived from the actinism of the solar rays.

These successive transfers of force through molecular changes and movements are therefore but expressions of dynamic evolution resulting from the collisions of gravitative or attractive forces with repulsive forces, in the matter of the celestial bodies. From which it results that the *origin* of all dynamic displays (no less

than their perpetual conservation) lies in the static affections either inherent in, or indelibly stamped upon all material elements. The derivative and convertible forms of energy should not, therefore, be confounded with the primordial and immutable forces resident in the molecule and its atoms—such as cohesive, affinitive, and gravitative tendencies or affections.

If molecular attractions and repulsions are really the parents of all dynamic energy, it seems wholly improbable that any form of such attraction or repulsion can ever be the offspring of dynamic energy. In other words, we must infer that such exhibitions of molecular attraction and repulsion as those of electricity and magnetism, however seemingly the product and correlatives of motion—as of friction or percussion, of light or heat, of chemical activity or gravitative fall, etc.—are really not so derived, but are to be regarded as being only unveiled or made manifest, from a previous condition of neutralization by a static equilibrium.

9TH MEETING. JUNE 24, 1871.

The President in the Chair.

Prof. A. HALL read a paper

ON ASTRONOMICAL PHOTOGRAPHY.

(This paper is published under the title, "On the Application of Photography to the Determination of Astronomical Data," in the American Journal of Science and Arts (3), *vol. ii., pp.* 25–30, *July,* 1871.)

(ABSTRACT.)

An account was given in this paper of what had been accomplished in applying photography to the determination of exact astronomical data. Reference was made to the labors of Messrs. Bond and De la Rue, and it was inferred: (1) that in the case of ordinary observations of double stars the photographic method presents no advantages over the direct method with a filar micrometer or heliometer; (2) that in the case of an eclipse of the sun, or of the transit of a planet, the results hitherto obtained are such as to give but little confidence in the accuracy of the photographic method. On account of the obvious advantages which the photographic method possesses over the observations of contact in the case of a transit of Venus, provided that the photographic observations can be rigorously and accurately re-

duced, attention was called to the importance of making early and complete experiments for determining the real value of this method.

Mr. E. B. ELLIOTT made a communication

ON THE STATISTICS OF THE BORROWING POWER OF THE UNITED STATES.

Mr. J. E. HILGARD made a communication

ON THE DISTRIBUTION OF THE POPULATION IN THE UNITED STATES.

10TH MEETING. SEPTEMBER 9, 1871.

The President in the Chair.

The evening was occupied by verbal communications from various parties.

11TH MEETING. SEPTEMBER 23, 1871.

The President in the Chair.

Prof. S. NEWCOMB read a paper

ON THE TRANSITS OF VENUS, PAST AND FUTURE.

Dr. THEODORE GILL made some remarks

ON ADDITIONS TO THE FISH FAUNA OF MASSACHUSETTS, DUE TO THE RESEARCHES OF PROF. S. F. BAIRD, U. S. FISH COMMISSIONER.

(*This communication was substantially as published under the title, "On recent additions to the Fish Fauna of Massachusetts," in the Proceedings of the American Association for the Advancement of Science, for* 1873, *B. pp.* 34–36.)

12TH MEETING. OCTOBER 7, 1871.

The President in the Chair.

Prof. A. HALL read a paper, illustrated by a diagram,

ON A CURVE OF THE FOURTH DEGREE.

(*This paper is published in the Educational Times, vol. xix.*)

(ABSTRACT.)

The curve to be considered arises from the solution of the following question, proposed in one of the English annuals: "Through the focus of an ellipse a right line is drawn cutting the ellipse in the points D and E, and at the middle point of D E an indefinite right line is drawn perpendicular to D E. It is required to find the form and area of the curve that this perpendicular always touches."

Taking the centre of the ellipse as the origin of coördinates, and its principal axes as the axes of reference, the equation of the perpendicular is

$$a^2y\,m^3 + (a^2x - c^3)\,m^2 + b^2y\,m + b^2x = o;$$

where a and b are the semi-axes of the ellipse, m the tangent of the angle which the line D E makes with the axis of x, and $c = a\,e$; denoting by e the eccentricity of the ellipse. The equation of the curve sought will be found by eliminating m between the preceding equation and its first derivative with respect to m. The result of such an elimination was called by the older mathematicians the *resultant* of the two equations, and in the phrase of modern algebra it is called the *eliminant*. Performing the elimination the equation of the curve is found to be—

$$4\,a^2b^4y^4 + (8\,a^4x^2 + 20\,a^2c^3x - c^6)\,b^2y^2 + 4\,x\,(a^2x - c^3)^3 = o. \quad (1)$$

Putting $h = a\,e^3$, and solving for y we find,

$$y = \pm\frac{a}{8\,b}\left(\sqrt{h} \pm \sqrt{h + 8\,x}\right)^{\frac{1}{2}}\left(3\sqrt{h} \mp \sqrt{h + 8\,x}\right)^{\frac{3}{2}} \quad (2);$$

where the upper and lower signs in the radical expressions must be taken together.

This elegant form for the value of y follows from this fact, that in the solution of (1) the terms in x retain a cubic form through two successive reductions. Equation (2) shows the form of the curve. It is confined to the limits $x = +h$, and $x = -\frac{h}{8}$; and has double points for these values of x. If we denote the area of the curve by A, we have

$$\frac{4\,b}{a}\,\mathrm{A} = \int_{-\frac{1}{8}h}^{h}\left(\sqrt{h} + \sqrt{h + 8\,x}\right)^{\frac{1}{2}}\left(3\sqrt{h} - \sqrt{h + 8\,x}\right)^{\frac{3}{2}}dx$$

$$-\int_{-\frac{1}{8}h}^{o}\left(\sqrt{h} - \sqrt{h + 8\,x}\right)^{\frac{1}{2}}\left(3\sqrt{h} + \sqrt{h + 8\,x}\right)^{\frac{3}{2}}dx.$$

Whence, performing the integrations and restoring the value of h, we have

$$A = \frac{c^3 e^3}{8 b} \pi.$$

Dr. B. F. CRAIG read a paper

ON THE FLUCTUATIONS OF THE TEMPERATURE OF THE HUMAN BODY.

(*This paper is published under the title, "Variations in the Temperature of the Human Body," in the American Journal of Science and Arts* (3), *vol. ii., pp.* 330–332, *Nov.* 1871.)

Mr. E. B. ELLIOTT presented an essay

ON THE NEW COINAGE OF JAPAN.

Mr. J. E. HILGARD read an essay

ON AN EXPONENTIAL FORMULA HAVING REFERENCE TO THE TOLERANCE ALLOWED AT THE U. S. MINT.

13TH MEETING. OCTOBER 21, 1871.

The President in the Chair.

Prof. BENJ. PIERCE read a paper

ON THE HEAT OF THE SUN.

The author stated that he desired to offer evidence confirmatory of Mr. Lane's views as to the gaseous constitution of the sun, holding that the outer limit was fixed by laws of heat.

Prof. W. HARKNESS made a communication

ON THE PHYSICAL CONSTITUTION OF THE CORONA OF THE SUN.

(*This paper is published in Washington Astronomical Observations*, 1869, *Appendix I., pp.* 82-89.)

Prof. JOSEPH HENRY made a communication

ON OBSERVATIONS MADE ON A JOURNEY TO CALIFORNIA;

this paper was illustrated by the rain-fall maps published by the Smithsonian Institution.

14TH MEETING. NOVEMBER 4, 1871.

Vice-President W. B. TAYLOR in the Chair.

This being the annual meeting for the election of officers, the Chairman announced the order of proceedings for the evening as determined on by the general committee. As the result of the election, Professor JOSEPH HENRY was unanimously re-elected President; Generals J. K. BARNES and M. C. MEIGS, and Messrs. HILGARD and TAYLOR, Vice-Presidents; Hon. PETER PARKER, Treasurer; Doctors GILL and CRAIG, Secretaries; and the following were elected members at large of the General Committee:—

S. F. BAIRD,	A. HALL,
T. L. CASEY,	A. A. HUMPHREYS,
J. H. C. COFFIN,	T. A. JENKINS,
E. B. ELLIOTT,	S. NEWCOMB,

J. J. WOODWARD.

No amendments to the Constitution having been made, the chairman announced that the annual address of the President was necessarily deferred until a subsequent meeting on account of his absence from the city.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON,

FOR THE YEAR 1871–1872.

PRESIDENT.

JOSEPH HENRY.

VICE-PRESIDENTS.

M. C. MEIGS,
J. E. HILGARD,
W. B. TAYLOR,
J. K. BARNES.

TREASURER.

PETER PARKER.

SECRETARIES.

B. F. CRAIG,
THEODORE GILL.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

S. F. BAIRD,
T. L. CASEY,
J. H. C. COFFIN,
E. B. ELLIOTT,
A. HALL,
A. A. HUMPHREYS,
S. NEWCOMB,
J. J WOODWARD,
G. H. ELLIOT.*

* Subsequently elected to fill a vacancy.

15TH MEETING. NOVEMBER 18, 1871.

The President in the Chair.

The meeting was opened by the delivery of the annual address of the President.

(*This Address will be found printed in full in the introductory portion of this volume.*)

A letter to Professor Henry from Dr. Bessels was read, dated on board the Polaris, August 16th, giving some account of the scientific operations of the North Pole Expedition up to that date.

Prof. W. HARKNESS read a paper

ON THE SPECTRUM OF ENCKE'S COMET, AND THE APPEARANCE OF TUTTLE'S COMET.

(*This paper is essentially as published in Washington Astronomical Observations*, 1870, *Appendix II., pp.* 25–49.)

Dr. B. F. CRAIG made a communication

ON APOTHECARIES' WEIGHTS AND MEASURES.

16TH MEETING. DECEMBER 2, 1871.

The President in the Chair.

Prof. A. HALL read a paper

ON THE ASTRONOMICAL PROOF OF THE EXISTENCE OF A RESISTING MEDIUM IN SPACE.

(*This paper is published in full in the American Journal of Science and Arts* (3), *vol. ii. pp.* 404–408, *Dec.* 1871.)

Prof. W. HARKNESS made a verbal communication regarding some singular results that he had lately arrived at by the study of the spectrum of Encke's comet.

(*The substance of this communication will be found in Washington Astronomical Observations*, 1870, *Appendix II., pp.* 25–49.)

Mr. JOSEPH HENRY read an eulogy

ON THE LIFE AND SCIENTIFIC LABORS OF THE LATE ALEXANDER DALLAS BACHE,

who was one of the founders of the scientific club from which this society took its origin.

(*This Eulogy is published in full in the Report of the Secretary of the Smithsonian Institution for* 1870, *Appendix, pp.* 90–106.)

17TH MEETING. DECEMBER 16, 1871.

The President in the Chair.

Mr. J. E. HILGARD read a paper

ON THE WESTWARD MOVEMENT OF THE POPULATION OF THE UNITED STATES.

(*This paper is published under the title, "The Advance of Population in the United States," in Scribner's Monthly Magazine, vol. iv., pp.* 214–218, *June,* 1872.)

Mr. C. S. PEIRCE read a paper

ON THE APPEARANCE OF ENCKE'S COMET AS SEEN AT HARVARD COLLEGE OBSERVATORY.

(*The substance of this communication will be published in the Annals of the Harvard College Observatory.*)

Mr. E. B. ELLIOTT made a communication

ON THE LOCUS OF THE POINT OF EQUAL ILLUMINATION BY TWO UNEQUAL LIGHTS TREATED BY THE QUATERNION ANALYSIS.

Mr. CLEVELAND ABBE read a letter from Mr. S. A. King, aeronaut, of Boston,

ON THE AERIAL CURRENTS OBSERVED IN FIFTY BALLOON ASCENSIONS.

The thanks of the Society were returned to Mr. King for his communication.

BALLOON ASCENSIONS

BY S. A. KING.

DATE.		PLACE OF ASCENT.	PLACE OF DESCENT.	TIME.	PARTIAL COURSES TOWARDS
1855.	Aug. 11.	Wilmington, Del.	Fleming's Hotel.	5 P.M.–6 P.M.	0, S.W., E., S., N.W.
1856.	June 16.	Wilkesbarre, Pa.	Bear Creek.	3.20–4.20 P.M.	0, E., S.E.
1856.	Oct. 9.	Providence, R. I.	Barrington.	10 A.M.	S.E., N., N.W.
1857.	May 19.	Providence, R. I.	North Scituate.	3 P.M.	0, W.
1857.	July 4.	Providence, R. I.	Pawtucket.	4.30 P.M.	N., S.E., N.E.
1857.	Aug. 11.	New Haven, Conn.	Stony Creek.	3.35–4.25 P.M.	E., E.S.E.
1857.	Oct. 2.	Norwich, Conn.	Canterbury, Conn.	3.45 P.M.	E., N.E.
1857.	Oct. 9.	Concord, N. H.	Tewksbury.	3.30–5.30 P.M.	S., S.E.
1858.	June 1.	Providence, R. I.	Smithfield.	6–7.30 P.M.	0, S.W., N.
1858.	July 4.	Lowell, Mass.	Tewksbury.	6.30–7.45 P.M.	S.E.
1858.	July 26.	Manchester, Mass.	Strafford.	5.20–7 P.M.	N., N.E.
1858.	Aug. 6.	Paterson, N. J.	New York City.	5–6.30 P.M.	S.E.
1858.	Aug. 25.	New Haven, Conn.	Fair Haven.	6–6.30 P.M.	E.N.E.
1858.	Aug. 26.	New Haven, Conn.	Ferryville.	1–3 P.M.	N.N.W.
1858.	Oct. 8.	Dover, N. H.	———	5 P.M.	E.
1860.	July 4.	Boston, Mass.	Groton, N. H.	6.15 P.M.–1 A.M.	0, W.N.W., W., S.W.
1863.	July 4.	Boston, Mass.	Deering, N. H.	6–11 P.M.	N.W.
1863.	July 5.	Deering, N. H.	Farmington, N. H.	4–7 P.M.	N.E.
1863.	Oct. 14.	Bangor, Me.	Hartt's Mills, N. B.	1–7 P.M.	N.E., S.E., E.
1864.	July 4.	Boston, Mass.	Long Island, Mass.	5.45–6.15 P.M.	E., S.E.
1865.	July 8.	Boston, Mass.	Melrose, Mass.	— —	0, W., S.E., W.
1868.	Sept. 14.	Rutland, Vt.	Mount Holly.	— —	S., S.W.
1868.	Oct. 2.	Claremont, N. H.	Springfield, N. H.	4.30–5.30 P.M.	N.E., E.
1868.	Oct. 3.	Springfield, N. H.	Newburn, N. H.	4–5 P.M.	S.
1868.	Nov. 21.	Taunton, Mass.	New Bedford, Mass.	2 P.M.	S.E., S.S.W.
1869.	July 5.	Buffalo, N. Y.	Warren Co , Pa.	4.15–11 P.M.	S.W., S.E.
1869.	Sept. 23.	Rochester, N. Y.	Webster Village.	4.40–5.50 P.M.	W., E.

DATE.		PLACE OF ASCENT.	PLACE OF DESCENT.	TIME.	PARTIAL COURSES TOWARDS
1869.	Oct. 19.	Rochester, N. Y.	Cazenovia, N. Y.	5.10–8 P.M.	E.
1869.	Dec. 10.	Atlanta, Ga.	Alpharata.	2.30–4.30 P.M.	N.W., N. of E., N.E.
1870.	Feb. 9.	Augusta, Ga.	Clarendon Co., S. C.	4.15–7.30 P.M.	N.E.
1870.	Mar. 10.	Augusta, Ga.	Bath Mills.	4.30–5.30 P.M.	E. of N.
1870.	May 5.	Charleston, S. C.	St. Thomas's Parish, S. C.	6.30–7.35 P.M.	N.E., N.
1870.	June 17.	Charlestown, Mass.	East Boston, Mass.	5–5.45 P.M.	N., E.
1870.	July 4.	Buffalo, N. Y.	Wright's Corners.	3.50–5.10 P.M.	N., E. of N.
1870.	Sept. 6.	Newburgh, N. Y.	Westport, Conn.	4.40–6.15 P.M.	S.E., E.
1870.	Sept. 16.	Barton, Vt.	Swift River, Me.	3–6 P.M.	E., S.E.
1870.	Sept. 30.	Troy, N. Y.	Guilderland.	4–5.30 P.M.	S., S.W., W.
1870.	Oct. 14.	Unionville, N. Y.	Warwick, N. Y.	5–6 P.M.	0, S.E., N.
1870.	Oct. 24.	Middletown, N. Y.	Millerton, N. Y.	5 45–6 P.M.	N., N.E.
1870.	Nov. 24.	Ellenville, N. Y.	Norwalk, Conn.	3.30–4.52 P.M.	N.E.
1871.	May 30.	Bridgeport, Conn.	Old farm.	6.05–6.30 P.M.	N.E.
1871.	June 17.	Charlestown, Mass.	Kingston, N. H.	5–8 A.M.	E. of N.E., N.E., N., N.W., N.
1871.	July 4.	Chelsea, Mass.	Westford, Mass.	7.30–11.30 P.M.	0, N.W.
1871.	July 8.	Meriden, Conn.	Meriden, Conn.	5–5.30 P.M.	E.
1871.	Aug. 28.	Rochester, N. Y.	Parma, N. Y.	4–5 P.M.	W., N.W., N.
1871.	Sept. 2.	Elmira, N. Y.	Hammond's Cor.	3.05–6.12 P.M.	S.W., N.E., S. by E.
1871.	Sept. 8.	Barton, Vt.	Kirby, Vt.	4.15–6 P.M.	0, S.E.
1871.	Sept. 20.	Cazenovia, N. Y.	W. Oneonta, N. Y.	3 30–5.30 P.M.	S.E.
1871.	Sept. 27.	Fitchburg, Mass.	Ayer, Mass.	4.15–5.50 P.M.	S.E., N.E.
1871.	Oct. 5.	Plymouth, N. H.	Bridgeton, Me.	4.18–5.45 P.M.	N.E., E., S.E.

In the last column each current as we ascend or descend, counts as one; the wind or lowest current is included: 0 indicates a sensible calm. In the next to the last column the time of ascent and, when possible, of descent is given.

WASHINGTON, Dec. 16, 1871.

GENTLEMEN: In the pursuit of professional duties as an aeronaut I have constantly endeavored to elevate my calling, and to utilize any knowledge I may have acquired in my voyages through the air. Possibly it may interest you to receive even a simple table of the directions of the aerial currents that have prevailed during my excursions. I have therefore attempted to compile such a table from some of the most interesting of the 162 voyages that I have made; and have the honor herewith to present it for your acceptance. The labor of compiling this table [pp. 36, 37,] has been considerable, owing to the desultory nature of the accounts that have been published, and to their being, as you well understand, scattered through hundreds of newspapers. The present table comprises but about fifty ascensions: if the Society deem it desirable, however, I should be pleased to complete this work, by adding, at some future time, similar tables for the remainder of my voyages.

It would afford me great satisfaction to learn that you deem this humble, and as I believe novel contribution to meteorology worthy of your acceptance.

Respectfully,

SAML. A. KING, Aeronaut, of Boston.

The following remarks were made by Mr. ABBE:—

In laying this communication before you I may state that I have examined Mr. King's Table of Fifty Balloon Voyages, and have formed the following synopsis, showing how many times each current has been recorded:—

COURSE.	NO. OF TIMES.
Calm	9
From the north	6
" northeast	8
" east	8
" southeast	9
" south	12
" southwest	20
" west	16
" northwest	17
Total	105

In this table each current is counted once, and a decided predominance of westerly currents is evident. This is in part explained by the fact that ascensions are made by preference only in settled pleasant weather, *i. e.* on the front side of an advancing area of high barometer. Ascensions are, however, also made generally in the afternoon, when on our Atlantic coast the summer afternoon sea breeze would tend to increase the number of easterly winds in the lowest stratum of air.

Mr. King's table gives the currents in the order of their superposition, beginning with the surface wind, and it is rare to find an upper current in a direction opposed to the lowest or surface wind—such generally deviate but 90° – 135° from each other. The ascensions have rarely exceeded ten thousand feet in altitude, and thus can give us an insight into the nature of only the lower system of currents that precede extended storms.

From seven balloon ascensions made on July 4, 1871, at different points in the United States, I have deduced the velocity of the upper currents as about four times that of the surface wind then prevailing.

18TH MEETING. JANUARY 13, 1872.

The President in the Chair.

Mr. W. HARKNESS read a paper

ON THE DENSITY OF THE HYPOTHETICAL RESISTING MEDIUM IN SPACE.

(*This communication is published in Washington Astronomical Observations,* 1870, *Appendix II., pp.* 33–38.)

Mr. T. GILL read a communication

ON THE TAPIR OF THE ANDES AND ITS ALLIED FORMS.

Mr. R. D. CUTTS presented a paper

ON THE MISAPPLICATION OF GEOGRAPHICAL TERMS, AS BEARING ESPECIALLY ON THE QUESTION OF THE INTERPRETATION OF THE FISHERY RIGHT TREATIES.

(ABSTRACT.)

Reference was made to the general misapplication, in the United States, of the term "creek" to fresh water streams and rivulets; to the long international dispute as to the point where the Rhine terminated, and especially to the authoritative determination of the mouth of the River St. Lawrence.

Under the late Reciprocity Treaty between the United States and Great Britain, made principally with a view to settle the question of the in-shore fisheries, a Joint Commission was appointed to examine the coasts of the North American British colonies and of the United States as far south as the 36th parallel, being over 6000 miles of coast, including indentations, and to designate the "*rivers and the mouths of rivers*." These were to be reserved from the common liberty of fishing, while "*bays, harbors, and creeks*" were free. Here was an immense field opened for the discussion and international interpretation of the above terms. The commissioners were directed to examine each "*place*" which could in any sense be considered as a "*river*," and if decided to be a river, then to agree upon a line which should mark the outer limit of its mouth. The number of "*places*" presented for examination on the Provincial coasts was 167, and 54 on the coast of the United States. Of these, 105 were declared to be rivers, and their mouths were designated on official charts. The Commission was in existence for a period of ten years, 1855 to 1866, although, owing to the late civil war, only about five years were strictly devoted to the duty assigned it. There was a large number of cases in which a difference of opinion was expressed. Some were reconciled, and in others an appeal was taken to an umpire, under the authority of the treaty.

As the U. S. Surveyor attached to the Commission, the examinations on the part of the United States were principally conducted by me, and it frequently occurred that special reports and long discussions became necessary as to the right of this or that "*place*" to be designated by this or that term. The designations found on the maps and charts were not considered as definitive authority. The only rule which we could adopt was that prescribed by international law, that terms employed in treaties should be interpreted according to the definition given of them by the science to which they belonged.

To show what divergence of opinion may be entertained in the discussion of such apparently simple questions, it may be stated that inlets of the sea, of greater or less extent, were called "*rivers*" by one party and "*creeks*" by the other; that "*bays*" of large size were claimed as the "*mouths of rivers*" on the ground that streams, inconsiderable in size, emptied into them; and that a "*river*" was not only an inland current of fresh water, but was one also when the inlet owed its waters almost entirely to the sea.

Of the many questions which arose, however, the most important was in regard to the line which should mark the outer limit of the River St. Lawrence. During the discussion which preceded the settlement of the Northeastern Boundary, the British Government indirectly claimed that the mouth of that river was defined by a line drawn from Cape Rozier to the Island of Anticosti, and thence to the Mingau Islands on the north shore, and until very lately the Gazetteers and many maps have assigned to it the same relative position. The British Commissioner under the Reciprocity Treaty presented a claim to the same line which, if it had been yielded to, would have excluded the fishermen of the United States from a part of the sea more extensive than the Bays of Chaleur, Fundy, Delaware, and Chesapeake put together. To meet this claim, the river from Quebec to the Gulf was examined, and an argument prepared, based upon the discharge of the inland current of fresh water; the parallelism or divergence of the banks; the freshets and their effects, the tides and currents; and the depth, specific gravity, and coldness of the water between the mouth of the Saguenay and the Island of Anticosti, which showed that the river and its mouth terminated perhaps at Red Island Bank, and certainly at Pr. de Monts. The area embraced within the two lines claimed respectively by the United States and Great Britain contained over 10,000 square miles of sea, valuable for its fisheries.

The British Commissioner finally yielded, and the outer limit of the mouth of the St. Lawrence was established by a line drawn from Pr. de Monts to Cape Chatte. The waters between that line and the Island of Anticosti constitute the northwest arm of the Gulf of St. Lawrence.

19TH MEETING. JANUARY 27, 1872.

The President in the Chair.

Mr. J. J. WOODWARD made some remarks

ON THE DESIRABILITY OF REPRODUCING PHOTOGRAPHS OF SCIENTIFIC OBJECTS, AND ESPECIALLY OF MAGNIFIED MICROSCOPICAL PREPARATIONS, IN A PERMANENT FORM BY SOME PHOTO-MECHANICAL METHOD.

(ABSTRACT.)

Like M. Alexander Agassiz,* he had recently tried both the Woodbury process, practised by Mr. John Carbutt, No. 1002 Arch Street, Philadelphia, and the Albertype method, used by Mr. E. Bierstadt, No. 902 Broadway, New York. The first had reproduced a negative representing an ovule *in situ* in a mammalian ovary magnified 400 diameters, and had furnished an edition of five hundred copies of excellent quality and great uniformity. These prints were cheaper and handsomer, as well as more permanent, than silver prints, but like them required careful mounting on good stiff card-board. Mr. Bierstadt had furnished proofs by his method from a negative representing a section of mammary cancer, also magnified 400 diameters. These proofs were quite equal to the Woodbury prints, and had the advantage of being on flexible paper suitable for binding. If the edition should turn out to be equal to the proofs, this method would certainly be the more desirable one, and would be a valuable aid to those who wish to obtain trustworthy representations of scientific objects. Dr. Woodward then exhibited the illustrations above referred to.

Mr. HENRY presented a report from Mr. E. J. Farquhar

ON CERTAIN REMARKABLE EFFECTS OF LIGHTNING.

Mr. B. F. CRAIG made some remarks

ON THERMOMETERS;

exhibiting a number of instruments of different kinds.

20TH MEETING. FEBRUARY 5, 1872.

The President in the Chair.

Mr. J. S. BILLINGS read a paper

ON SOME MINUTE FUNGI,

illustrated by numerous drawings and specimens.

* Application of Photography to Illustrations of Natural History, by Alexander Agassiz.

(ABSTRACT.)

After a brief description of the system of classification of the microscopic fungi at present in use, attention was called to the fact that this system is in many respects imperfect, and can only be improved by learning the life history of these organisms. Various forms of culture apparatus and growing slides were exhibited, including those of Hallier, De Bury, Maddox, and the speaker.

The nature and mode of propagation of bacteria were explained, and the results of a series of experiments on spontaneous generation, being a repetition of those described by Dr. H. C. Bastian, were exhibited. These results did not correspond with those given by Dr. Bastian. Solutions of turnip placed in tubes sealed while boiling were allowed to cool, and were then reheated—by sets of three—to 100°, 120°, 140°, 160°, 180°, 200°, 210°, 220°, and 240° F. respectively. No signs of change or life appeared in the tubes heated to 180° and upwards. The fluid in a part of the tubes not heated to 180° became turbid from bacteria. The results of attempts to cultivate bacteria in various forms of growing slides and on different substrata were shown. In no case could development into Hypho- or Physo-mycetous forms be considered as proven. It is probable that the microzymes include things of very diverse origin, properties and powers which cannot be distinguished from each other by any power of the microscope at our command.

Mr. B. F. CRAIG submitted two thermometers of new pattern, and explained

A METHOD OF VERIFYING WITH EXACTNESS THE INDICATIONS OF A THERMOMETER.

Mr. W. B. TAYLOR stated certain views entertained by himself on the subject of the Aurora Borealis, promising a more elaborate communication at an early date.

21ST MEETING. FEBRUARY 24, 1872.

The President in the Chair.

Mr. W. B. TAYLOR presented a communication

ON THE AURORA.

(ABSTRACT.)

The two generally recognized circumstances of an annual periodicity in the number and brilliancy of auroral displays, and of the great elevation—of several hundred miles—frequently attained by their arches and streamers, an elevation to which our atmo sphere can hardly be supposed to extend, having led to a question whether extra-terrestrial matter in the form of cosmical dust or gaseous rings (similar to the August and November rings of meteors) might not be concerned in the phenomenon, by being periodically grazed by our planet or its atmosphere—causing the discharge of electrical brushes—the recent elaborate Memoir of Prof. Lovering on Auroras was consulted with much interest, to see what light, if any, might be thrown upon the speculation by his carefully tabulated results.

As the summing up of a large number of local observations, two independent tables, classified by months, are given; the first table being derived from catalogues reaching back some 575 years from 1864, and embracing an aggregate of 9885 observations; and the second or supplementary table covering about 50 years, reaching down to 1868, and embracing an aggregate of 2497 observations (amounting together to 12,382 observations): both show when presented graphically the same characteristics, namely, two very notable maxima of frequency, in March and October, and one very remarkable minimum in June—the second minimum in December being much less marked. By plotting a third curve giving the summation of the two curves, this is still more clearly presented: the total maxima of March and October being respectively 1436 and 1341; and the two minima of June and December being respectively 455 and 1090.

Mairan in 1754, from a tabulated catalogue of 1441 auroras, estimated the number occurring at our perihelion (in the early part of January), as being about seven times that at our aphelion (in the early part of July). Prof. Lovering, however, from his much larger collection, has pointed out that the two minima agree very nearly with our solstices, and the two maxima with our equinoxes. The great minimum of June occurs when our nights are shortest; but after making due allowance for this, there is still a very marked deficiency observable.

Plotting in a corresponding manner the monthly frequency of meteoric displays, in three separate curves, representing, first, an aggregate of 1358 observations collected by Biot from the Chinese annals (reaching back considerably beyond the Christian era); secondly a table of Arago, comprising an aggregate of 813 observations of noted fire-balls; and thirdly, a table of Baumhaur, of aerolites and fire-balls observed previous to 1845, amounting to an aggregate of 767, we see that while there is a rough correspondency in these three curves, they show apparently no relation whatever to the curves of aurora frequency.

The inference deducible, therefore, from this brief and hasty survey is that no indication is afforded by Prof. Lovering's results, of an extra-terrestrial matter playing any part in the auroral discharge; and that we must regard the phenomenon as a terrestrial one, notwithstanding the reach of 400 and 500 miles, which has been ascribed to the luminous beams.

Following this communication, remarks were made as follows:—

Mr. E. B. ELLIOTT thought that the frequency of auroras had a simple relation to the change in the length of the earth's radius vector. Auroras occur most frequently when the earth is most rapidly approaching to or receding from the sun—that is, at times when the radius vector is most rapidly changing its length. To illustrate this view Mr. Elliott presented a table comparing by months the frequency of auroras, with the monthly differences in the logarithms of the earth's radius vector. The monthly periods of maximum frequency of auroras being in March and October, while the periods of the most rapid increment and decrement of the logarithm of the radius vector were likewise respectively in the months of March and October.

The data employed by Mr. Elliott to show the frequency of auroras embraced from ten to twelve thousand observations, as given in the general catalogue of auroras published by Professor Lovering.

Mr. C. ABBE stated that he had lately carefully studied the valuable tables of Prof. Lovering, and had, moreover, during the past year, systematically collated all the observations of auroras accessible to him, with the tri-daily weather charts of the Army Signal Office, and had arrived at a firm conviction that the aurora stood in an intimate relation to the condition of the earth's atmosphere; that, in fact, although its ultimate cause might be ever acting—might be cosmical, and might therefore be subject to periods of one, eleven, and fifty-five years—yet on the other hand that cause could not produce its visible effect, the aurora, except in certain conditions of the earth's atmosphere, and that therefore certain remarkable relations existed between auroral phenomena and terrestrial storms, &c., some of the details of which he then briefly indicated.

Mr. S. Newcomb thought the difficulties of parallactic determinations rendered the estimate of the elevation of auroral streamers quite unreliable, and that the height had been greatly exaggerated, the British observations not indicating much over 100 miles. In regard to the tabulated monthly frequency, as auroras were mainly seen between 8 and 10 o'clock, the fact that these hours fell in twilight during the months of June and July in the northern countries might of itself account for the small number of auroras seen in those months.

Mr. J. E. Hilgard perfectly agreed with Mr. Newcomb. On one or two occasions Prof. Henry, with others, had attempted to obtain the parallax of notable and characteristic auroras, but while a distance could, of course, be assigned from the angles given by any two observers, so soon as a third observation was combined there was no accord, and no parallax possible. In addition to the uncertainty of the monthly numbers from the changed lengths of the evenings, it appeared that no allowance had been made for the average cloudiness of nights, which would also be found to have an annual law, and the omission of which would introduce another element of uncertainty and source of error in comparing monthly numbers.

Mr. J. Henry said that the phenomena of the aurora were evidently electrical, and as such there were two facts to consider: first, the electrical discharge; and second, the matter illuminated by the discharge. He had, during a visit to Lake Superior a few years ago, repeated an experiment in which a beam of the auroral light, concentrated by a small concave mirror, fell on a paper on which were letters written with sulphate of quinine, and which became visible as in the experiments with the same invisible writing when illuminated by a discharge of electricity.

He had also made an observation on the effect produced by the aurora on the needle of a galvanometer, one end of the wire of which was connected with the water pipes, and the other with the gas pipes of the city. In the exhibition of the aurora, on one occasion the needle was deflected 90°, and was only stopped by two pins placed at this degree to prevent further motion.

A similar effect was always observed when a flash of lightning took place within the visible horizon of Washington.

From these results it would appear that the aurora acts inductively in the same way as a discharge of electricity acts.

Mr. THEO GILL made a communication

ON A TUNNY NEW TO THE AMERICAN COAST.

22D MEETING. MARCH 9, 1872.

The President in the Chair.

Mr. J. J. WOODWARD made a communication

ON THE USE OF MONOCHROMATIC SUNLIGHT AS AN AID TO HIGH POWER DEFINITION.

(*This paper is published in full in the American Naturalist*, 1872, *August, vol. vi. p.* 354.)

(ABSTRACT.)

In this paper Mr. Woodward recounted the various attempts hitherto made to employ monochromatic sunlight for the illumination of the higher powers of the microscope, and claimed that sunlight passed through a solution of the ammonio-sulphate of copper, as first recommended by Prof. J. W. Draper for photographic purposes, affords especial advantages for the definition of difficult lined tests, such as Amphiplema pellucida, and the Noberts plate. Several convenient methods of applying this mode of illumination to microscopical objects were given in detail.

Mr. J. E. HILGARD made some remarks

ON THE AURORA OF FEBRUARY 4TH.

He said that since the last meeting of the Society accounts had been received of the aurora borealis of February 4th having been observed not only throughout Europe, but also in Egypt and India. He had previously mentioned the account he had received of its appearance in Texas, and had now to add that it had likewise been seen in California. It appeared thus to have extended successively over at least two-thirds of the northern hemisphere. It would be of great interest now to collect information from vessels at sea between Europe and America, in order to learn whether the phenomenon had been continuous.

The magnetic variation was observed by Prof. Quimby at Dartmouth College, New Hampshire, throughout the day and evening. The greatest disturbances, reaching 5° of declination, were observed between 12 and 1 o'clock in the afternoon, when the auroral display was at its height in England, while the disturbances were far less marked in the evening, when the phenomenon occupied the entire southern sky at the place of observation.

Mr. HENRY explained that greater deflections might be expected from electric currents on one side of the magnet, than when taking place on both sides, when the fluctuations would only be produced by differences.

Mr. HILGARD also read an account of auroral phenomena visible between the observer and the horizon in the background, reported by an observer of the Army Signal Corps in Indianapolis, confirmatory of similar observations reported by Simpson, Lesley, and others.

Mr. POWELL took the present occasion to report an appearance of the same character, witnessed by himself.

Mr. J. W. POWELL then made some

REMARKS ON THE STRUCTURAL GEOLOGY OF THE VALLEY OF THE COLORADO OF THE WEST.

The lower portion of the Valley of the Colorado lies but little above the level of the sea, except where short mountain ranges rise out of the plain. Proceeding up the Valley of the Colorado past the Virgin to a distance of fifty or sixty miles, we find that the country to the north is elevated from fifteen hundred to two thousand five hundred feet above the lower plain. The boundary separating the plain below from the country above, is marked by a line of cliffs stretching across the valley from east to west, in many places vertical for hundreds of feet, and in passing from the plain below to the country above, these so obstruct the way that it becomes necessary to search for some pass by which the ascent can be made, and such passes are infrequent.

His remarks were restricted to that portion of the valley lying between this line of cliffs and the line of the Union Pacific Railroad.

This region lies from five to eight thousand feet above the level

of the sea. On the east, the rim of the basin of the Colorado is set with snow-clad mountains, rising from eight to nearly fifteen thousand feet above the level of the sea; and on the west, by mountains and plateaus rising from eight to twelve thousand feet. The grand anticlinal folds cross this portion of the valley from east to west. The most northern of these is marked by the eastern Uintah Mountains. This range, well defined on the west, extends to the east across Green River until it becomes involved with the profound transverse poles of the Rocky Mountains. The up-turned axis of the second pole crosses the Valley of the Colorado about twenty miles south of the junction of the Grand and Green, and is lost in the Wasatch plateau in the west. Its eastern extension has been examined to a distance of about thirty miles from the river. The up-turned axis of the third fold crosses the valley along a line a little south of the Colorado-Chiquito. If the first fold had been lifted up without denudation *pari passu*, its summit would be about 27,000 feet above what is now the bed of the river, or about 32,500 feet above the level of the sea. Under the same supposed conditions, the summit of the second fold would be about 22,000 feet above the level of the sea; and the third fold about 32,500 feet.

This region is also traversed by folds, the axes of which are in lines running in a north and south direction. These folds have displaced the strata from 50 to 1500 feet; are variable in length, the shortest observed being about 12 miles, the longest about 110, and are scattered irregularly over the country.

Mr. Powell then discovered and discussed the system of drainage that would have obtained had these folds been lifted up simultaneously and without denudation, until the displacement of the formations was completed.

He then explained the system of drainage actually found. This led to a classification of the valleys or lines of drainage.

Valleys have been observed to occur running in a direction along the synclinal axis of folds; others along the anticlinal axes. Other valleys were observed running in a direction with the folds, but located between the up-turned and down-turned axes. The slope on the side next to the anticlinal axis conforming in a general way with the dip of the formations. The slope on the side next to the synclinal axis was usually found more abrupt, and

marked by a line of cliffs in which are exposed nearly vertical sections of formations denuded from the other side of the valley.

Most of the larger valleys of this region are of this character. These are called paraclinal valleys. This gives three classes of valleys having a general direction the same as that of the folds, all called longitudinal valleys. Still other valleys were found cutting across folds. These were called diaclinal. Others were found running in a direction against the dip of the rocks and entering into anticlinal or paraclinal valleys. These were called contraclinal.

Valleys were also found running with the dip of the formations, and called conclinal. So three classes of valleys were found having a general direction transverse to the axes of the folds, and all called transverse valleys. And the three classes of longitudinal valleys, with the three classes of transverse valleys, were designated as simple valleys. Other valleys were found which in parts of their courses might be referred to one of these classes, and in other parts to some other class. These were called complex valleys. And again, valleys were found having their main course simple or complex, and having lateral valleys belonging to other classes. These were called compound valleys.

The Valley of the Colorado itself is formed by the coalescing of two or more distinct systems of drainage. And this was called a Grand Valley. A tabular statement of the proposed classification was made, as follows:—

I Order: Valleys conforming to geological folds.

II Order: Valleys in formations not folded.

III Order: Valleys superimposed upon folded formations.

The first order was divided into three sections:—

First Section: Simple Valleys.

Second Section: Complex Valleys.

Third Section: Compound Valleys.

The first section, *i. e.* Simple Valleys, was separated into two divisions:—

Division First: Longitudinal Valleys.

Division Second: Transverse Valleys.

Of the Longitudinal Valleys there are three classes:—

First: Synclinal.

Second: Anticlinal.

Third: Paraclinal.

Of the Transverse Valleys there are three classes:—

First: Diaclinal.
Second: Contraclinal.
Third: Conclinal.

Grand Valleys are formed by the coalescing of distinct systems of drainage, and their several parts may belong to two or more of the different orders of valleys.

The Valleys of the Colorado, Mississippi, and St. Lawrence are examples.

In considering this system of drainage in the Valley of the Colorado, and the conditions under which it was formed, the following inferences are made:—

First: That the elevation of the folds above the sea proceeded but little faster than their denudation by rains and rivers. And that it is not probable that the summits of the mountains were ever much higher than at present.

Second: That these folds were not elevated simultaneously, but progressively, against some fixed point of dry land. It is probable that this fixed point of dry land for the east and west folds was to the south, and that for the north and south folds was to the east.

Third: The axis of an emerging fold may fall on the land or the sea side of the shore line, and thus determine the direction of valleys.

Fourth: The direction of valleys is sometimes determined by the lithological character of the formations.

A brief reference was made to the influences modifying the contour of the valleys, to the general amount of erosion, and to the distribution of the debris of such erosion, which is not carried away to the sea.

23D MEETING. MARCH 23, 1872.

The President in the Chair.

Mr. J. H. SAVILLE presented a paper

ON THE NEW JAPANESE COINAGE.

Mr. S. F. BAIRD presented a communication

ON THE DECREASE OF FISH ON THE SOUTHERN COAST OF NEW ENGLAND.

(This paper is published in Report of the U. S. Fish Commissioner for 1871–72.)

Following the paper of Mr. Baird, Mr. GILL made some remarks on the relations of the fishes of Massachusetts to those of other regions, affirming that the fauna of the State was composed of two elements, a northern and southern, the boundaries of which were, on the whole, tolerably well defined by Cape Cod. The forms found north of the Cape were in great part represented by corresponding or identical species in northern Europe, while those inhabiting the southern waters were mostly nearly allied to forms characteristic of the Caribbean Sea, or merely wanderers from that region. In respect to the influence of the blue fish on the supply of scup, he was inclined to believe that while the pounds are in operation, the diminution of the scup would be accelerated in constantly increasing ratio up to a certain point, the disturbance of the equilibrium being due to the difference of habits of the two species. The scup is rather a shore fish, while the blue fish is an open water species and exempt in greater proportion from capture in pounds; therefore, the ratio of prey to the enemy would be constantly diminished by the capture of a disproportionate number of the former.

24TH MEETING. APRIL 6, 1872.

The President in the Chair.

Mr. C. E. DUTTON read a paper

ON THE MEASUREMENT OF THE PRESSURE DEVELOPED BY THE EXPLOSION OF GUNPOWDER IN FIREARMS.

Mr. S. NEWCOMB made a communication

ON THE POSSIBILITY OF A UNIVERSAL ATMOSPHERE.

Mr. B. F. CRAIG exhibited a recently invented apparatus for the generation of ozone from the flame of the Bunsen burner.

25TH MEETING. APRIL 20, 1872.

The President in the Chair.

Mr. J. E. HILGARD made some remarks

ON HINDOO ARITHMETIC.

The same gentleman also presented as a second communication, one

ON THE RECORDING SYSTEMS OF THE TRANSATLANTIC CABLES.

Mr. JOSEPH HENRY made a verbal report

ON THE EXPENDITURE OF THE INCOME OF THE BACHE FUND FOR 1872.

Prof. J. C. WATSON, of Ann Arbor, Mich., made some remarks

ON THE DISCOVERY OF NEW PLANETS HAVING ESPECIAL REFERENCE TO THE ASTEROIDS.

Major KING, of New York City, made a communication

ON THE FATIGUE OF METALS.

26TH MEETING. MAY 4, 1872.

The President in the Chair.

Mr. WM. FERREL read a paper

ON THE EFFECTS OF WINDS AND BAROMETRIC PRESSURE ON THE TIDES AT BOSTON, AND ON THE MEAN LEVEL OF THE SEA.

(This paper is published in substance in the American Journal of Science, 3d series, Vol. V., pp. 342–347, *May*, 1873.)

(ABSTRACT.)

This paper related to some recent results which the author had obtained from the discussion for the Coast Survey of the tidal observations of Boston harbor. These results pertain mostly to the effects of changes of the barometric pressure, and of the winds upon the mean level of the sea. The author found that the variation of mean level was inversely to that of the height of the mercurial column as seven inches of mean level to one inch

of mercury very nearly. He said that theory, upon the hypothesis that the fluid would always have time to assume the state of a static equilibrium of the forces, would require that there should be a change of about 13.5 inches of mean level to one of mercury, and that a change of about ten inches of mean level to one inch of mercury had been obtained from the Liverpool tidal observations. He thought the small coefficient in the case of the Boston tides might be due to the shallowness of the water in the harbor and channel leading to the tide gauge, since the shallower the water, the greater must be the velocity of displacement in assuming the state of static equilibrium belonging to a given change of mean level in any given time, and consequently in this case the periods of the oscillations of mean level may be too short for the fluid to assume at any time the state of static equilibrium even approximately.

With regard to the effect of the winds upon the mean level of the ocean in the harbor, he found that the winds from N. W. around by S. W. to S. E. depressed the mean level, the maximum depression belonging to S. W. winds where the depression is but little more than two inches for a wind of average force. On the contrary, the winds from the N. W. around by the N. E. to the S. E. raise the mean level about the same amount, the maximum belonging to the N. E. winds.

We also find that S. W. winds depress the barometer about one-twentieth of an inch on the average, and N. E. winds raise it about the same amount. The monthly averages of the barometer were least in March and greatest in September, the range being about 0.2 inch, and the average very nearly 30.10 inches at the level of tide water.

Mr. C. E. DUTTON gave

AN ACCOUNT OF SOME RECENT EXPERIMENTS ON DIFFERENT KINDS OF GUNPOWDER AT FORTRESS MONROE.

Prof. PORTER, of Belfast, Ireland, gave an account of

RECENT EXPLORATIONS IN SYRIA UNDER THE AUSPICES OF THE PALESTINE EXPLORATION FUND.

27TH MEETING. MAY 18, 1872.

Vice-President W. B. TAYLOR in the Chair

Mr. G. K. GILBERT read a communication

ON CERTAIN RECENT GEOLOGICAL AND GEOGRAPHICAL RESEARCHES IN ARIZONA AND NEVADA.

(ABSTRACT.)

I had the pleasure to accompany, as geologist, the party sent out last year in command of Lieut. Geo. M. Wheeler by the U. S. Engineer Department to survey and describe certain little known portions of Arizona and southern Nevada. The chief work was geographical, and the corps of topographers was so large that they were enabled to separate in detachments, connecting their work at stated points. In this manner original geographical data, adequate to the mapping of the mountain ranges, were obtained over an area of 83,000 square miles.

While there is great diversity in the character of the country, a division is possible into two contrasted types—the cordillera and the plateau, the disturbed and the undisturbed, the ridged and the furrowed.

The surface of Nevada belongs entirely to the first of these types. From the Wahsatch range to the Sierra Nevada the face of the land is corrugated in a system of ridges, few of them long or lofty, but so frequent that one who should cross the country in a right line would meet one, on the average, in every twenty miles. These ridges coincide approximately with meridians and are as near parallel as are the Wahsatch range and the Sierra Nevada. They are composed of (1) strata from Silurian to Trias, more or less altered and uplifted, resting against (2) irruptive granite and syenite. Over and about them are great accumulations of volcanic rocks, but of these I will only say at present that they are omnipresent in the field of our exploration. Further south, in southeastern California and the southwestern half of Arizona, there is a prolongation of the same system, with a deflection of the prevailing trend from N. and S. to N. W. and S. E., and a further change from slightly altered to highly metamorphic strata. A peculiarity of the cordillera country, due to climate, is, that little of it finds drainage to the ocean, but it is partitioned into numerous independent valleys, each gradually filling with *debris* from the mountains.

The plateau region, on the contrary, is well drained. Its strata are unbroken and either horizontal or lifted in gentle undulations. Denudation has scored it with deep chasms, and these, with the tabular character of its uplands, are its distinguishing features. To this system belong northeastern Arizona and great areas in New Mexico and Utah. The limit of this district is an important geographical and geological line, and its determination for 350 miles in Arizona is one of our most valuable geographical results.

This section [referring to a diagram] shows the rocks cut by the Colorado River from the foot of the Grand cañon to the Virgin range. The notable points it illustrates are the profound displacement of the strata that accompanied the upheaval of that range, and the evidence afforded of two epochs of folding. The

strata that in one place are horizontal, and in another are tilted at so high an angle against the mountain, are Palæozoic—Carboniferous at top and in chief part, but comprising some Devonian and, possibly, some Silurian beds also. They rest unconformably on a system of plicated crystalline schists that received their contortion in pre-Devonian and probably pre-Silurian time, while the disturbance which uplifted the Virgin range occurred in Secondary time and produced the entire cordillera system of the Great Basin. Prof. Whitney and Mr. King have been able to refer the latter definitely to the Jurassic epoch, and they have also discriminated a third (Tertiary) system of folds of which the principal manifestations are without our field.

The subject of the distribution of sedimentary rocks in the "Great West" is quite in its infancy In most of the ranges are folded more than one group of rocks, and whenever a complete geological map of the Cordilleras shall have been made, it will show as complicated a labyrinth as that which represents the arrangement of strata in the Appalachians. It is only by rudely tentative lines that the few general features already recognized can be represented. To begin with the lowest: there is a broad area in southwestern Arizona and adjacent parts of California in which the ranges are built of highly metamorphic rocks—chiefly crystalline schists—and granite. In these schists no fossils have been found, and I shall apply to them provisionally the title Azoic—without, of course, intending thereby to declare them destitute of life. They may prove equivalent to the Laurentian and Huronian of Canada, and they may belong, in part at least, to the Silurian. At present it appears most probable that they are pre-Silurian, and have never been covered by other sediments, but rose above the Silurian ocean (as Newberry has already suggested), a continent coeval with that of the Laurentian hills. Northward from this Azoic area stretches a band of Silurian and Devonian, broad in southern Nevada and narrow in northern, marking an early axis of continental upheaval. East and west it is flanked by Carboniferous, and then Trias, in belts trending north and south—not regularly, indeed, but rudely symmetrical about the axis of elevation in central Nevada, and these in turn are succeeded both east and west by Cretaceous and later beds.

[Mr. Gilbert illustrated the distribution of the rocks by a chart, and exhibited advance sheets of the map engraved to accompany the preliminary report of the expedition, then in press.]

Dr. Van Sant, of San Francisco, explained the theory of his new method of lighting gas jets by electricity, and exhibited the apparatus invented by himself for that purpose.

Mr. W. Harkness read a letter from Capt. Tupman, of the British Navy, communicating the results of observations made in India on the recent solar eclipse.

28TH MEETING. JUNE 1, 1872.

The President in the Chair.

Mr. G. K. GILBERT read a paper

ON SAND SCULPTURE IN THE WEST.

(ABSTRACT.)

The author exhibited a number of specimens illustrative of the work done by sand propelled by wind, and of the similar work of sand propelled by water, accompanied by a few explanatory remarks, in the course of which he called attention to the fact that in river erosion the chief erosive agent is moving sand, and the function of the water is the propulsion of the cutting particles and the transportation of the eroded material, and to the fact that in arid regions, where aqueous denudation is at a minimum, its place is taken by the denudation of wind-borne sand, for which arid conditions are most favorable.

Mr. J. H. C. COFFIN and Mr. J. J. WOODWARD read portions of letters recently received from Dr. B. A. Gould, Director of the National Observatory at Cordoba, Argentine Republic, giving an account of the progress of that institution.

Mr. J. J. WOODWARD made a verbal communication on the Woodbury photo-relief process, exhibiting Woodbury-prints of photographs reproduced by Mr. J. Carbutt at the establishment of the American Photo-relief Printing Company for the Medical History of the War. In his opinion the minute details of the negatives were more faithfully preserved by this method than by the Albertype process, and it was therefore preferable when natural objects of delicate texture were to be represented. It was also well suited for reproducing photo-micrographs, as shown by several proofs, which he also exhibited.

Mr. E. FRISBY read a paper

ON A SERIES FOR THE DETERMINATION OF THE NUMBER EXPRESSING THE RATIO OF THE CIRCUMFERENCE TO THE DIAMETER.

(This paper is published in part in the Messenger of Mathematics, December, 1872.)

This calculation has generally been made by means of the series—

$$\tan^{-1}x = x - \frac{x^3}{3} + \frac{x^5}{5} - \frac{x^7}{7} + \text{\&c.}$$

If we put $\tan^{-1}x = \frac{\pi}{4}$ or $\frac{\pi}{6}$, we have $x = 1$ or $\frac{1}{\sqrt{3}}$; the former is inconvenient because it converges too slowly, and the latter on account of the radical expression.

Many other series may be deduced by resolving the arc into the sum of two or more arcs whose tangents are known; thus—

$$\frac{\pi}{4} = \tan^{-1}1 = \tan^{-1}\frac{1}{2} + \tan^{-1}\frac{1}{3} \qquad (a)$$

$$= 2\tan^{-1}\frac{1}{2} - \tan^{-1}\frac{1}{7} \qquad (b)$$

$$= 2\tan^{-1}\frac{1}{3} + \tan^{-1}\frac{1}{7} \qquad (c)$$

$$= \tan^{-1}\frac{1}{2} + \tan^{-1}\frac{1}{5} + \tan^{-1}\frac{1}{8} \qquad (d)$$

$$= 4\tan^{-1}\frac{1}{5} - \tan^{-1}\frac{1}{239} \qquad (e)$$

$$= 4\tan^{-1}\frac{1}{5} - \tan^{-1}\frac{1}{70} + \tan^{-1}\frac{1}{99} \qquad (f) \text{ \&c. \&c.}$$

Any of the numbers become exceeding complicated when raised to high powers; but these equations have all been used. Clausen used (*c*) for computing π, and Dase used (*d*) to 200 decimals. Machin used (*e*) and Rutherford (*f*). Shanks has also lately used Machin's series for computing π to 707 decimals.

We have also—

$$\text{Sin}^{-1}x = x + \frac{x^3}{3} + \frac{1.3}{2.4.5}x^5 + \text{\&c.}$$

$$\text{or, } x = \sin x + \frac{\sin^3 x}{2.3} + \frac{1.3}{2.4.5}\sin^5 x + \text{\&c.} \qquad (g)$$

Formula (*c*) gives

$$\pi = 2\tan^{-1}\frac{1}{3} + \tan^{-1}\frac{1}{7} = 2\sin^{-1}\frac{1}{\sqrt{10}} + \sin^{-1}\frac{1}{\sqrt{50}} \qquad (h)$$

On account of the numbers 10 and 50 in the denominators, this form is more simple than any of the previous ones, but inconvenient on account of the radical and the complicated coefficients.

But Gauss, in his "*Disquisitiones generales circa seriem infinitam*," &c., has given another series, which can be used very conveniently.

In order to demonstrate this series we shall put $2x$ for x in equation (g), and it then becomes

$$2x = \sin 2x + \frac{1}{2.3}\sin^3 2x + \frac{1.3}{2.4.5}\sin^5 2x + \&c.$$

$$= \sin 2x \left\{1 + \frac{1}{2.3}\sin^2 2x + \frac{1.3}{2.4.5}\sin^4 2x + \&c.\right\}$$

where the nth term within the bracket is of the form

$$c_n \sin^{2n-2} 2x = c_n 2^n (\sin^{2n-2} x)(1 - \sin^2 x)^{n-1},$$

and therefore only involves even powers of $\sin x$, and the series can consequently be put into the form

$$2x = \sin 2x \{1 + a\sin^2 x + b\sin^4 x + c\sin^6 x + \&c.\}$$

a, b, c, &c. being at present unknown.

In order to determine them we shall differentiate this series, and we have

$$2 = 2\cos 2x\{1 + a\sin^2 x + b\sin^4 x + c\sin^6 x + \&c.\}$$
$$+ \sin 2x\{2a\sin x + 4b\sin^3 x + 6c\sin^5 x + \&c.\}\cos x$$
$$= 2(1 - 2\sin^2 x)(1 + a\sin^2 x + b\sin^4 x + c\sin^6 x + \&c.)$$
$$+ 2(1 - \sin^2 x)(2a\sin^2 x + 4b\sin^4 x + 6c\sin^6 x + \&c.)$$

whence equating coefficients we have

$$3a - 2 = o \quad \text{whence } a = \frac{2}{3}$$

$$5b - 4a = o \quad \text{"} \quad b = \frac{2.4}{3.5}$$

$$7c - 6b = o \quad \text{"} \quad c = \frac{2.4.6}{3.5.7} \quad \&c.\ \&c.$$

therefore

$$2x = \sin 2x\left(1 + \frac{2}{3}\sin^2 x + \frac{2.4}{3.5}\sin^4 x + \frac{2.4.6}{3.5.7}\sin^6 x + \&c.\right)$$

or $x = \sin x \cos x \left(1 + \frac{2}{3}\sin^2 x + \frac{2.4}{3.5}\sin^4 x + \frac{2.4.6}{3.5.7}\sin^6 x + \&c.\right)$

which is a very convenient series.

If $\sin x = \frac{1}{\sqrt{10}}$, $\cos x = \frac{3}{\sqrt{10}}$ and $\sin x \cos x = \frac{3}{10}$;

If $\sin x = \frac{1}{\sqrt{50}}$, $\cos x = \frac{7}{\sqrt{50}}$ " $\sin x \cos x = \frac{14}{100}$;

but $\pi = 8 \sin^{-1} \frac{1}{\sqrt{10}} + 4 \sin^{-1} \frac{1}{\sqrt{50}}$ from equation (h), which, when substituted in this last equation, gives

$$\pi = 2.4 \left\{ 1 + \frac{2}{3}\left(\frac{1}{10}\right) + \frac{2.4}{3,5}\left(\frac{1}{10}\right)^2 + \frac{2.4\,6}{3.5.7}\left(\frac{1}{10}\right)^3 + \&c. \right\}$$

$$+ .56 \left\{ 1 + \frac{2}{3}\left(\frac{2}{100}\right) + \frac{2.4}{3.5}\left(\frac{2}{100}\right)^2 + \frac{2.4.6}{3.5.7}\left(\frac{2}{100}\right)^3 + \&c. \right\}$$

$$= 2.4\,x + .56\,y = 2.4\left(x + \frac{7}{30}y\right).$$

If we now examine the terms, we see that the coefficient of the n^{th} term is deduced from that of the $(n-1)^{th}$ term by multiplying it by $\frac{2n-2}{2n-1} = 1 - \frac{1}{2n-1}$; therefore if we subtract the $\left(\frac{1}{2n-1}\right)^{th}$ part of itself from each term and remove the result one figure to the right, we obtain the next term of the first series, &c.

For the second series we start our computation with the same number and multiply each succeeding term in the first series by the consecutive powers of $(\frac{2}{10})$, and lastly multiply the result by $\frac{7}{30}$ and add it to the first series.

The computation in this manner gives every term positive, and by computing the result to thirty places of decimals and taking every time the nearest unit in the last place, it gives the thirtieth figure accurate.

The computation to 30 places of decimals stands thus:—

RATIO OF CIRCUMFERENCE TO DIAMETER = π TO 30 DECIMALS.

FIRST SERIES.	SECOND SERIES.
2.4	2.40
.16	.0320
.0128	.000512
.001097142857142857142857142857	87771428571428571428571 43
97523809523809523809523810	1560380952380952380952 38
8865800865800865800865801	2837056277056277056277
818381618381618381618382	52376423576423576424
76382284382284382284382	977693240093240092
7188920883038530097354	18403637460578637
681055662603650219749	3487004292530 70
64862444057490497119	6641914271487
6204233779412134507	127062707802
595606442823564913	2439603990
57354694494121066	46984966
5537694640811683	907296
535905932981776	17560
51966635925506	341
5048187489906	7
491174999018	
47858076827	
4669080666	
456049739	
44591530	
4364277	
427521	
41914	
4112	
404	
40	
4	
2.574004435173137547211236914869	

2.432520936071381533934599150330
7

30)17.027646552499270737542194052310

.567588218416655691251406468410

First series = 2.574004435173137547211236914869

π = 3.141592653589793238462643383279

The series (b)

$$\frac{\pi}{4} = 2\tan^{-1}\frac{1}{2} - \tan^{-1}\frac{1}{7} = 2\sin^{-1}\frac{1}{\sqrt{5}} - \sin^{-1}\frac{1}{\sqrt{50}}$$

could also be used, and the second series is exactly the same as the first, with the consecutive figures removed one figure further to the right, but it does not converge so rapidly as the preceding.

29TH MEETING. JUNE 15, 1872.

The President in the Chair.

Mr. A. HALL read a paper

ON THE EXPERIMENTAL DETERMINATION OF THE RATIO OF THE CIRCUMFERENCE TO THE DIAMETER, BASED ON THE PRINCIPLES OF THE CALCULUS OF PROBABILITIES.

Mr. THEO. GILL communicated a memoir by Mr. F. B. MEEK

ON THE DISCOVERY OF NEW SPECIES OF FOSSIL PLANTS FROM ALLEGHENY CO., VA., WITH SOME REMARKS ON THE GEOLOGY OF THAT STATE.

Mr. A. HALL read a paper entitled

A HISTORICAL NOTE ON THE METHOD OF LEAST SQUARES.

(*This communication is referred to in Nature (London)*, 1872, *vol. vi. pp.* 101 and 241.)

Mr. O. STONE read a memoir

ON THE DETERMINATION OF THE ERRORS OF A PROVISORY CATALOGUE OF FUNDAMENTAL STARS.

30TH MEETING. JUNE 29, 1872.

The President in the Chair.

Mr. S. NEWCOMB made a verbal communication

ON THE PROGRESS OF THE CONSTRUCTION OF THE NEW TELESCOPE FOR THE NAVAL OBSERVATORY.

(*The substance of these remarks is published in Scribner's Monthly Magazine, October*, 1873.)

Mr. W. B. TAYLOR made some verbal comments on a recent publication

ON OUR PRESENT KNOWLEDGE OF THE PLANET JUPITER.

31ST MEETING OCTOBER 5, 1872.

The President in the Chair.

Mr. E. B. ELLIOTT read a letter from Dr. Curtis, giving an account of Prof. Hayden's progress in the survey of the Western Territories.

Mr. S. NEWCOMB made a second communication

ON THE PROGRESS OF THE CONSTRUCTION OF THE NEW TELESCOPE FOR THE NAVAL OBSERVATORY.

(The substance of these remarks is published in Scribner's Monthly Magazine, October, 1873.)

General W. T. SHERMAN, by invitation of the Chair, gave an account of his recent visit to Egypt.

32D MEETING. OCTOBER 19, 1872.

The President in the Chair.

Mr. JOSEPH HENRY made a communication

ON THE FLUCTUATIONS OF THE RIVER NILE.

Mr. J. H. C. COFFIN exhibited, with some comments,

MAPS PREPARED BY G. W. HILL FOR USE IN CONNECTION WITH THE TRANSIT OF VENUS IN DECEMBER, 1874.

(These maps are published with text as Part II. of Papers relating to the Transit of Venus.)

Mr. CHARLES S. PEIRCE made a communication

ON STELLAR PHOTOMETRY.

(This paper will be published in the Annals of the Harvard College Observatory.)

Mr. E. B. ELLIOTT communicated some details of an investigation made by him

ON THE ADJUSTMENT OF CENSUS RETURNS.

33D MEETING. NOVEMBER 2, 1872.

The President in the Chair.

This being the annual business meeting, the Secretary read the order of proceedings as determined on by the General Committee, and also read the names of those entitled to vote in the election of officers. As the result of the election, Mr. JOSEPH HENRY was declared unanimously re-elected as President. For Vice-Presidents there were elected Messrs. J. E. HILGARD, W. B. TAYLOR, M. C. MEIGS, and J. K. BARNES. As Treasurer, Mr. PETER PARKER was unanimously re-elected. Mr. B. F. CRAIG having declined a renomination for the Secretaryship, there were elected as Secretaries Mr. T. GILL and Mr. S. NEWCOMB. As members at large of the General Committee, there were elected—

N S. LINCOLN,	S. F. BAIRD,
E. B. ELLIOTT,	J. H. C. COFFIN,
A. HALL,	J. J. WOODWARD,
B. F. CRAIG,	A. A. HUMPHREYS,

T. L. CASEY.

On the close of the election the President, in a few remarks, reviewed the condition and activity of the Society during the past year.

34TH MEETING. NOVEMBER 16, 1872.

The President in the Chair.

Mr. THEODORE GILL read a paper

ON THE HOMOLOGIES OF THE SHOULDER GIRDLE OF FISHES.

(*The substance of this communication was published in the author's "Arrangement of the Families of Fishes," pp. xiii.–xix.*, 1872, *and, under the title, "On the Homologies of the Shoulder Girdle of the Dipnoans and other Fishes," in the "Annals and Magazine of Natural History"* (*London*), *4th series, v.* 11, *pp.* 173–178, *March*, 1873.)

Mr. W. HARKNESS read a paper

ON SOME MEASUREMENTS OF HEIGHTS BY A POCKET ANEROID.

Mr. J. E. Hilgard explained the construction of the new aneroid constructed by Goldschmidt, illustrating his remarks by exhibiting one of the instruments in question.

Dr. B. F. Craig read a communication

ON THE WATER SUPPLY OF CITIES.

35th Meeting. November 30, 1872.

The President in the Chair.

General W. T. Sherman gave an account of his travels in Turkey and the Caucasus, recounting numerous observations of general interest made by him in those countries.

36th Meeting. December 11, 1872.

The President in the Chair.

The President stated that the meeting had been called a few days earlier than usual for the purpose of introducing Professor John Tyndall, of the Royal Institution, London, but that the proceedings would be those of an ordinary meeting. The President then read a paper

ON CERTAIN ABNORMAL PHENOMENA OF SOUND IN CONNECTION WITH FOG SIGNALS.

Prof. Tyndall made some remarks on this subject, citing certain cases of abnormal phenomena which had come under his own notice.

Mr. S. Newcomb gave an account of

THE PROCEEDINGS OF THE COMMISSION TO ARRANGE FOR THE OBSERVATION OF THE NEXT TRANSIT OF VENUS.

Mr. W. B. TAYLOR read a paper

ON WAVES, MOLECULES, AND ATOMS.

(ABSTRACT.)

Of the five elements necessary to complete the physical theory of light and heat undulations, three are known with a tolerable degree of certainty and accuracy; to wit, the *velocity* of propagation, the *length* of the wave, and the varying *rapidity* corresponding thereto. The two remaining elements—the average *amplitude* of the wave, and the *form*, or character of excursion of a wave particle—are undetermined.

Mr. M. Ponton has recently* attempted to estimate the amplitude of the ethereal wave by a comparison of the velocities of light and sound, and the relative energies of elasticity involved, making it of the order of 500 thousand millions to the wave-length, or 2000 billion to the inch; and making the amplitude proportional to the wave length. Unfortunately, this estimate takes no account of the momentum or mass of the vibratory medium, an element which we have no means of determining.

The dynamical theory of heat as applied to gases, teaches us that their isolated constituent molecules, separated from their mutual cohesive attractions, are at all temperatures, in a state of motion and collision, not indeed of actual contact, but of impingement upon each other's dynasphere of repulsion; the energy of which repulsion is estimated by Maxwell as being inversely as the fifth power of the distance.

From computations made by Mr. Waterston in 1857, by Mr. Stoney in 1868, and by Sir William Thomson in 1870, all tolerably concordant, we may accept the order of magnitude of the molecules as being in the neighborhood of the 200 millionth of an inch. The conjoined calculations of Clausius and Maxwell have rendered it probable that at ordinary temperature and pressure the average length of free excursion of the molecules of a gas between collisions, is about 300 thousand to the inch, the mean velocity at the rate of 1640 feet per second, and the frequency of collisions, consequently, about 6000 million per second. These motions are, however, exceedingly variable and irregular, both in the lengths and in the velocities of the excursions; and therefore take no part in impressing the ethereal medium, or in the production of light.

Each molecule is a remarkably constant and complex structure, consisting of an aggregation of parts which may be provisionally called "atoms," and which are in regular or rhythmical movement among themselves; and it is in this motion of the material atoms that the luminiferous waves have ever their origin. We

* Quarterly Journal of Science, July, 1871, i. 349.

thus learn that for the mean wave-length of light (about the 45 thousandth of an inch), there are about 500 billion of these atomic oscillations or revolutions in one second; so that, brief as is the interval of molecular excursion from neighbor to neighbor, about 83 thousand of these undisturbed internal pulsations occur in the interval. There is thus a constant exchange and tendency to equalization of *vis-viva* between the atomic vibrations, and the molecular flights producing them; and from the very nature of the mode of transfer by successive irregular or variable impacts, we are forced to conceive these atomic motions as variable elliptical orbits, the straight line and the circle being the maximum and minimum limits of elliptic eccentricity. The *radiation* of heat is always of atomic origin, and is transmitted with the velocity of light. The *conduction* of heat is always of molecular origin, and is communicated through material contacts or impacts, and with very great slowness, from the time required to diffuse motions with such extreme irregularity and prevalent obliquity of impacts.

Knowing the mean period of the atomic orbits, if the average velocity communicated to them at collisions equalled the average velocity of the molecular flights, the average magnitude of the orbit might easily be computed at about the 25,000 millionth part of an inch, or the 500 thousandth of a mean wave-length. But there is reason to believe that the average velocity is much less than 1640 feet per second, and the mean diameter of the orbit correspondingly smaller; since of the total or aggregate *vis-viva*—external and internal—of the molecules, constituting the specific heat of a gas, more than half belongs to the translatory motion. That the orbit constituting the width of the wave must be exceedingly small, is evident from the very wide range of variation to which it is subject within the molecular magnitude. The large luminous orbits undoubtedly exceed the smallest by hundreds of thousands of times, or probably as sixteen miles exceeds one inch. The amplitude of the waves is as the square root of the intensity (which may vary a billion times), and inversely as the distance of the origin.

The conclusion arrived at respecting the two unknown elements of the luminiferous wave motion is, that the form is probably an elliptical orbit (as in the case of liquid waves), and that its diameter—constituting the amplitude of the wave—is for light of ordinary brilliancy, probably of an order approaching the billionth of an inch.

37TH MEETING. DECEMBER 21, 1872.

The President in the Chair.

Mr. CHARLES S. PEIRCE, of Cambridge, made some remarks

ON THE COINCIDENCE OF THE GEOGRAPHICAL DISTRIBUTION OF RAINFALL AND OF ILLITERACY, AS SHOWN BY THE STATISTICAL MAPS OF THE NINTH CENSUS REPORTS.

(ABSTRACT.)

The author called attention to the striking resemblance between the map showing the distribution of illiteracy (the percentage of population unable to read or write) in the United States, given in the Report of the Census of 1870, and the map showing the distribution of rainfall during the three winter months published in Mr. Schott's reduction and discussion of the Smithsonian observations of that element. Mr. Peirce suggested as a possible explanation for the resemblance that the copious winter rains would produce agricultural plenty, which in its turn would favor indolence.

Mr. T. GILL read a paper

ON THE SCOMBROCOTTUS SALMONEUS OF PETERS.

Mr. J. E. HILGARD read a report

ON THE PROCEEDINGS OF THE INTERNATIONAL METROLOGICAL COMMISSION.

38TH MEETING. JANUARY 4, 1873.

The President in the Chair.

Prof. J. R. EASTMAN read a paper on

A COMPARISON OF THE THERMOMETERS USED TO DETERMINE THE CORRECTION FOR ATMOSPHERIC REFRACTION AT THE U. S. NAVAL OBSERVATORY.

(ABSTRACT.)

The discrepancies between the two thermometers used in the observations with the transit circle and the standard thermometer

led him to place another thermometer on the west side of the observing room and make a series of comparisons with the standard.

Thermometer A was suspended on the outside of the northern wall of the observing room in a double tin box shaped like the frustrum of a pyramid with a base 30 inches square.

Thermometer B was placed on the northern wall near the roof, and just outside the eastern side of the vertical opening in the northern wall of the observing room.

Thermometer C was attached to a rod depending from the roof almost directly over the transit circle, and was nearly on the same level as the object-glass when the instrument was pointed at the zenith.

The standard thermometer D was in the meteorological observatory, situated southeast of the observing room.

Thermometer	A	was	7.5	feet above the floor of the observing room.
"	B	"	16.5	" " " "
"	C	"	13.5	" " " "
"	D	"	4.2	" the ground on the lawn.

From	A to B	the distance was	11.0 feet.
"	A to C	" "	23.5 "
"	B to C	" "	21.5 "
"	A to D	" "	about 37 yards.

Sixteen comparisons were made between May 8 and July 16, 1870, and the most marked differences were as below:—

The maximum difference between	A and B	was	4°.0, July 15.9
" " " "	A and C	"	6 .5, May 12.0
" " " "	A and D	"	7 .1, July 15.9
" " " "	B and C	"	4 .0, June 22.4
" " " "	B and D	"	4 .9, July 16.0
" " " "	C and D	"	6 .6, July 15.9

From the following equations giving the greatest positive and negative differences the ranges of differences are obtained:—

$A - B = +4°.0$ Range $4°.0$	$B - D = +4°.9$ $= -1°.6$ Range $6°.5$
$A - C = +6°.5$ $= -3°.0$ Range $9°.5$	$C - D = +6°.6$ $= -2°.1$ Range $8°.7$
$B - C = +4°.2$ Range $8°.2$ $= -4°.0$	

Representing the mean of the readings of the exterior and interior thermometers, A and C, by M, we have the maximum value of $M - D = 6°.9$, and the minimum value $= 1°.1$. Also the maximum value of $M - B = 3°.8$.

Assuming the temperature indicated by the standard thermom-

eter to be correct, it will appear that refractions computed for 65° zenith distance by means of temperatures obtained from thermometer A are liable to be in error 1″.7, by thermometer B 1″.2 and by thermometer C 1″.6.

At a zenith distance of 82° the error from computing from thermometer A might be 5″.0, from B 3″.4, and from C 4″.6.

Other combinations would show similar differences, though in one or two cases of less magnitude; all, however, indicating a marked uncertainty in the determinations of refractions at considerable zenith distances, even in a fixed observatory.

A communication was received from Mr. T. ANTISELL, now at Yokohama,

ON THE METEOROLOGY OF JAPAN.

39TH MEETING. JANUARY 18, 1873.

The President in the Chair.

Mr. R. D. CUTTS read a paper

ON THE RESULTS OF ASTRONOMICAL OBSERVATIONS AT SHERMAN STATION, WYOMING TERR.

(ABSTRACT.)

The author gave an account of the origin, organization, and results of an expedition for scientific purposes made during the past summer, to Sherman in Wyoming Territory, of which he had the general charge under the instructions of the Superintendent of the Coast Survey.

The expedition was authorized by Congress at the suggestion of the American Association for the Advancement of Science, and had for its principal object an astronomical reconnaissance to determine the advantages to be gained in the observation of celestial phenomena by an elevation of the instrument above nearly one-third and the densest portion of the atmosphere. The opportunity was also taken to determine the exact latitude and longitude of the station, and to make a series of meteorological observations with a view to obtain some idea of the climatic condition of the elevation, and of the fitness of the locality for an extended class of astronomical observations. For these purposes the party consisted of a full corps of observers, provided with

instruments of the most approved character. The special class of observations called for were made by Prof. C. A. Young, of Dartmouth College, with the 12-foot equatorial and spectroscopic apparatus belonging to that institution.

The astronomical station was on the high ground near Sherman depôt, which is situated on the line of the Union Pacific Railway where it crosses the summit of the Black Hills, the most eastern range of the Rocky Mountains. The latitude was 41° 07′ 49″.5; the longitude west from Greenwich, 105° 23′ 11″; and height above the sea-level, 8335 feet.

Three temporary observatories were erected; one for the transit and latitude instrument, chronograph and telegraphic apparatus; a second for the meteorological instruments and observers; and the third and largest for the equatorial telescope. The observations were commenced in June and continued through July and part of August, the meteorological class having been made hourly, day and night, for a term of sixty days.

The longitude was determined by telegraphic exchange of clock-beats, on seven nights, with Assistant A. T. Mosman at Salt Lake City, Utah Territory, the intermediate distance being 500 miles. *The latitude* was determined by 110 observations on nineteen pairs of stars for differences of zenith distance.

Cistern Barometers.—

The highest mean of 24 hours was on	June 21st,	22.488
The lowest mean " "	June 30th,	21.977
Mean height of the 60 day set	. . .	22.281

The tropical hours were found to be 3 A. M. and 5 P. M. for the periods of daily minima, and 9 A. M. and 10 P. M. for the daily maxima. The hourly oscillations at night were not half so great as during the day. The barometric column was in all cases reduced to the freezing point.

Fahrenheit Thermometers.—The temperature of the air varied from 32° to 81°.6, the mean of the day and night being 56°.5. The hour of lowest temperature was 4 A. M., or about half an hour before sunrise, and of the highest at noon. The mean temperature of the night, between sunset and sunrise, was 50°.3, and of the day, 60°.2.

Solar Maximum Radiation Thermometers.—The maximum excess of temperature of the solar rays, divested of the effects of vapor and wind, above the temperature as given by the thermometers in the shade but otherwise freely exposed, was tolerably uniform, the average being 73°.

Wet-bulb Thermometers.—The relative humidity varied from a comparative absence of vapor to complete saturation. Its mean value was 0.463. During many days and nights, the degree of moisture in the air was not one-tenth of what it could have held—showing the dryness of the atmosphere—while at other times, but very rarely, the mean of the day was 0.900.

Evaporation.—The mean of the maximum evaporation of water during the 24 hours for eleven days, was six times greater than when the water was kept in the shade and protected.

Wind.—The general direction of the wind was from the quarter between N. W. and S. W., or from the mountains to the plains. The proportion from this quarter during the 60 days was 53 per cent.; from S. W. to S E. 25 per cent.; from S. E. to N. E. 12 per cent.; and from N. E. to N. W. 10 per cent. The general direction of the upper strata, as inferred from the movements of the upper clouds, was from N. W. to S. E. The wind was, at times, very strong, and frequently continued so day and night, constituting, as a rule, dry gales. Its velocity ranged from two miles to 43.7 per hour, there being few occasions on which it was entirely calm. The velocity per day varied from 167 to 760 miles, the average being 332 miles.

Rain.—The total quantity of rain which fell, including melted snow and hail, amounted to 2.55 inches.

Electrometer.—The observations for atmospheric electricity taken on 25 days show that the kind and intensity followed no general law; that positive electricity prevailed during the forenoon; negative in the afternoon, and about equally divided at noon; while the omission of results at certain hours and days during the same period shows that in clear and even partially clear weather, no trace of electricity was discovered, or if it existed, it was too feeble in amount to be detected by the instrument. Three hundred quarter inch sparks per minute were frequently noted during a thunder storm, or during the prevalence of a storm in the mountains to the westward.

Casella Hypsometers.—The mean of 14 determinations of the boiling point, on eleven days, was 197°.33, the mean corrected barometer for the hours of observation standing at 22.309.

The weather during the 72 continuous days which the party spent at Sherman was characterized by excessive cloudiness; by high winds from the N. W. and W.; by the suddenness with which the sky would be obscured and then become clear again; by the various directions in which different cloud strata would be moving at the same time; and by a positive uncertainty in regard to the duration of any favorable opportunity for astronomical observations. The altitude was probably a little too great, as the summit was frequently enveloped in a fog or passing cloud when it was clear weather a few miles lower down the mountain slopes.

The dryness of the air, so much to be desired in astronomical work, may be illustrated by the following facts: rapid evaporation; unusual and almost immediate shrinkage of newly cut lumber; no perspiration; no animal putrefaction, and no mould. The dew-point was invariably lower than the temperature of the air, and hence there was no dew. The absolute amount of moist-

ure was no doubt greater than usual, and yet it was 50 per cent. less than at St. Louis.

When the sky was entirely free from clouds, the firmament was indescribably brilliant—and so much more than at the sea level that the value, to the unassisted eye, of the increased transparency of the atmosphere was assumed at one, if not two magnitudes. Stars of the fifth magnitude were observed for determination of time before sunset with the telescope of the small portable transit, and the companion to Polaris, ninth magnitude, was distinctly visible at night with a reconnoitring telescope of 2¼ inches aperture. The greater steadiness and definition were very apparent during the observations made on sixty different time stars.

Prof. Young states in his report to the Superintendent of the Coast Survey that hundreds of small stars which he had never seen at Hanover, came out distinctly, and that he thought a majority of the stars rated in the British Association Catalogue of the seventh magnitude, were fairly within reach of the naked eye. As an expression of the value of the elevation, he gives it as his deliberate opinion that at Sherman the $9\frac{4}{10}$ inch object glass of the 12 foot equatorial was just about equal to a 12 inch glass at the sea-level. The discoveries made by Prof. Young of new lines in the spectrum of the chromosphere, and his observations on the spectra of solar spots and on solar storms, have been published by him.

While the reconnaissance adds another proof of the advantages to be derived by so great an elevation of the telescope, the Meteorological Register clearly intimates that a more favorable position than Sherman should be searched for, in view of any proposition to erect a permanent observatory on the Rocky Mountains to be devoted to the advancement of astronomical science and discovery.

A full report of the expedition, illustrated with tables and diagrams, will appear in the Annual Report of the Superintendent of the Coast Survey for 1872.

Mr. R. Keith read a paper

ON ACHROMATIC OBJECT GLASSES.

Mr. G. A. Otis made a communication

ON FRACTURES OF THE INNER TABLE OF THE CRANIUM.

Mr. T. Gill made some remarks

ON THE HOMOLOGIES OF THE ARM IN FISHES, AND THE DEVELOPMENT OF THE HUMERUS IN GANOIDS.

40TH MEETING. FEBRUARY 1, 1873.

The President in the Chair

Mr. BENJ. ALVORD read a paper

ON THE HABITABILITY OF THE ELEVATED PLATEAUS OF THE WEST.

Professor BENJAMIN PEIRCE, of Cambridge, made some remarks

ON THE THEORIES OF THE NATURE OF COMETS' TAILS.

(*The substance of this communication had been previously communicated to the American Academy of Arts and Sciences, at Boston.*)

Mr. E. B. ELLIOTT made a communication

ON LIFE AND ANNUITY TABLES, BASED ON THE CENSUS OF 1870.

41ST MEETING. FEBRUARY 15, 1873.

The President in the Chair.

Mr. M. YARNALL read a paper

ON THE GENERAL STAR CATALOGUE PREPARED FROM THE OBSERVATIONS AT THE NAVAL OBSERVATORY SINCE 1845.

(*The substance of these remarks is published in Washington Observations,* 1871, *Appendix III.*)

Mr. W. HARKNESS made a communication

ON THE POWER NECESSARY TO DRIVE THE PENDULUM OF AN ASTRONOMICAL CLOCK.

42D MEETING. MARCH 1, 1873.

The President in the Chair.

Mr. E. FOOTE read a paper

ON THE LAWS OF CONDENSATION OF AQUEOUS VAPOR IN THE ATMOSPHERE.

Mr. C. E. DUTTON presented a memoir

ON THE CAUSES OF THE ELEVATIONS AND THE SUBSIDENCES OF THE EARTH'S SURFACE.

Mr. JOSEPH HENRY delivered the first of a series of discourses

ON ATMOSPHERIC ELECTRICITY.

43D MEETING. MARCH 15, 1873.

The President in the Chair.

Mr. E. FOOTE read a communication

ON A PROPOSED METHOD OF OBSERVING ASTRONOMICAL TRANSITS.

Dr. A. J. WOEIKOFF, of St. Petersburgh, read a paper

ON METEOROLOGY IN RUSSIA.

(*This paper is published in full in the Annual Report for* 1872 *of the Secretary of the Smithsonian Institution, pp.* 267–298.)

Mr. JOSEPH HENRY delivered a second discourse

ON ATMOSPHERIC ELECTRICITY.

44TH MEETING. MARCH 29, 1873.

The President in the Chair.

The PRESIDENT read a letter from General Lefroy, Governor-General of the Bermudas, relating to the changes of sea-level at Bermuda, and their relations to health.

Mr. E. B. ELLIOTT made a communication

ON INTERNATIONAL COINAGE.

Mr. E. FRISBY read a paper

ON A GREGORIAN CALENDAR.

(*The following is the calendar proposed by Mr. Frisby.*)

GENERAL GREGORIAN CALENDAR.

SUN.	MON.	TUES.	WED.	THUR.	FRI.	SAT.	SUN.	MON.	TUES.	WED.	THUR.	FRI.	SAT.	SUN.	MON.	TUES.	WED.	THUR.	FRI.	SAT.	SUN.	MON.	TUES.	WED.	THUR.	FRI.	SAT.
JANUARY.							APRIL.							JULY.							OCTOBER.						
				1	2	3				1	2	3	4				1	2	3	4					1	2	3
4	5	6	7	8	9	10	5	6	7	8	9	10	11	5	6	7	8	9	10	11	4	5	6	7	8	9	10
11	12	13	14	15	16	17	12	13	14	15	16	17	18	12	13	14	15	16	17	18	11	12	13	14	15	16	17
18	19	20	21	22	23	24	19	20	21	22	23	24	25	19	20	21	22	23	24	25	18	19	20	21	22	23	24
25	26	27	28	29	30	31	26	27	28	29	30			26	27	28	29	30	31		25	26	27	28	29	30	31
FEBRUARY.							MAY.							AUGUST.							NOVEMBER.						
												1	2							1	1	2	3	4	5	6	7
1	2	3	4	5	6	7	3	4	5	6	7	8	9	2	3	4	5	6	7	8	8	9	10	11	12	13	14
8	9	10	11	12	13	14	10	11	12	13	14	15	16	9	10	11	12	13	14	15	15	16	17	18	19	20	21
15	16	17	18	19	20	21	17	18	19	20	21	22	23	16	17	18	19	20	21	22	22	23	24	25	26	27	28
22	23	24	25	26	27	28	24	25	26	27	28	29	30	23	24	25	26	27	28	29	29	30					
29							31							30	31												
MARCH.							JUNE.							SEPTEMBER.							DECEMBER.						
1	2	3	4	5	6	7		1	2	3	4	5	6			1	2	3	4	5			1	2	3	4	5
8	9	10	11	12	13	14	7	8	9	10	11	12	13	6	7	8	9	10	11	12	6	7	8	9	10	11	12
15	16	17	18	19	20	21	14	15	16	17	18	19	20	13	14	15	16	17	18	19	13	14	15	16	17	18	19
22	23	24	25	26	27	28	21	22	23	24	25	26	27	20	21	22	23	24	25	26	20	21	22	23	24	25	26
29	30	31					28	29	30					27	28	29	30				27	28	29	30	31		

1700, etc.	1800, etc.	1900, etc.	2000, etc.	YEARS.																	
Sun.	Fri.	Wed.	Tues.		5	11	16	22		33	39	44	50		61	67	72	78		89	95
Mon.	Sat.	Thur.	Wed.	0	6		17	23	28	34		45	51	56	62		73	79	84	90	
Tues.	Sun.	Fri.	Thur.	1	7	12	18		29	35	40	46		57	63	68	74		85	91	96
Wed.	Mon.	Sat.	Fri.	2		13	19	24	30		41	47	52	58		69	75	80	86		97
Thur.	Tues.	Sun.	Sat.	3	8	14		25	31	36	42		53	59	64	70		81	87	92	98
Fri.	Wed.	Mon.	Sun.		9	15	20	26		37	43	48	54		65	71	76	82		93	99
Sat.	Thur.	Tues.	Mon.	4	10		21	27	32	38		49	55	60	66		77	83	88	94	

(*N. B.—The list of week days printed at the top of this table should be copied on a separate slip of paper and used as a movable slide.*)

DIRECTIONS.

Look for the day of the week at bottom answering to the year and century; put this day on the movable slide first to the left, *i. e.* opposite January 4th, and we have a complete calendar for the year; for leap years in January and February use the vacant space above the year, and for the rest of the year use the proper number. Leap year is known by being immediately below the vacant space; the presidential inauguration year is to the right of the vacant space, etc. For centuries those only are leap years which are divisible by 4, as 2000. The calendar repeats itself every 400 years.

Mr. F. M. Endlich read a memoir

ON MINERALOGICAL SYSTEMS.

Ever since the attention of naturalists was directed towards minerals, the necessity of a system of classification for them has been felt.

Aristotle already (384–322 a. Chr. n.) classified all minerals into ὀρυκτά (stones) and μεταλλευτά (ores), the former as owing their origin to the agency of humid vapor, the latter to dry gases. Theophrast, and later Pliny, describe a number of minerals, many of which may be recognized by the description, but no attempt was made by either of them to assign them their respective positions in any definite system. The Arabian physician Avicenna (about 1010 p. Chr. n.) separated the minerals into four classes, viz., *stones*, *inflammable fossils*, *salts*, and *metals*. About the same time Abul-Kihan-Albirouny took the specific gravity of a number of minerals, but no knowledge of any more important investigations from that time has reached us. Basil Valentin is said to be the author of a little book published in the German language about the year 1500, but this is most likely the work of several German miners combined. Here we find for the first time names like Quartz, Vries, Spat, &c. Georg Agricola, in his "De Natura Fossilium" (1546) has copied these names, and appears to be well acquainted with form, cleavage, hardness, gravity, color, lustre, &c. of a large number of minerals. Later, in 1760, the first impulse to study the *form* of minerals more carefully was given by the Danish mineralogist Bartholin, who measured and calculated the angles of the transparent Iceland double-spar. After this time considerable attention was paid to the crystallographic part of mineralogy, the knowledge of which received a very valuable addition by the assertion of the Dane Steno in 1669, that in rock crystal, in spite of unsymmetrical development of the faces, *the angles were constant*. Investigations of this kind, carried on by different mineralogists, tended to develop the interest in the crystalline forms of minerals, and in 1735 Carl Linné was the first naturalist who based a classification of the minerals on their crystallographic character, in his "Systema Naturæ, sive tria regna." He still labored under the old prevailing idea, that the salts chiefly produced crystalline forms, and therefore called them "*patres*," as creating the crystals in the "*matrices*." He distinguished in his classification :—

Petræ, rocks; *Mineræ*, minerals; and
Fossilia, petrefactions.

Through these views *Romé de Lisle* was led to the study of crystallography, and afterwards *Hauy*, in his "Essai d'une Thérie sur la Structure des Crystaux" 1784, and 1801 in his "Traité de Mineralogie" elevated crystallography to a special science. The works of these excellent investigators had a great influence

on the direction which mineralogy took in France, traces of which may to-day yet be observed in the character of their books upon that subject.

A new era was started by *Werner*, who introduced a *natural system*, and showed his views very clearly in a little book that appeared in 1774. He recognizes the minerals by exterior characteristics—lustre, fracture, color, hardness, sound, &c.—and even touch, temperature, weight, and taste help him to discriminate. His system was first published by Emmerling in 1793, and later by Hoffmann and Breithaupt, 1811–1813. He paid great attention to the crystallographic forms of minerals, and very carefully described them. In his scholar, Prof. Weiss, of Berlin, his system received a great support, and Weiss, as an excellent crystallographer, paid the necessary attention to that branch of mineralogical science. Mohs, also a scholar of Werner, in the same way followed the method of a natural classification, going so far as to almost entirely neglect the chemical composition of minerals. Haidinger made many valuable investigations regarding the physical qualities of minerals. C. F. Naumann, in his "Elemente der Mineralogie," 1846, took the same course, differing, however, from Weiss in the crystallographic part. Quenstedt, a scholar of Weiss, adopted the classification of minerals as proposed by him in his "Handbuch der Mineralogie," 1854, and his text-book, together with that of Naumann, are at the present time most extensively in use at institutions of learning throughout Germany.

At the same time, parallel with the work done by Werner and others, a new basis for the classification of minerals had been adopted by several mineralogists; it was the chemical basis. Wallerius of Sweden, in his "Mineralkicket," 1747, and Axel von Cronstedt in his "Fôrsôk til Mineralogie," 1758, proposed a classification of minerals based upon their chemical constituents. In 1782 Bergman published the "Sciagraphia regne Mineralis," which is generally regarded as the first chemical system. Vauquelin and Klaproth furnished a large number of very valuable analyses, so that the necessity of combining chemistry with mineralogy became more and more apparent. Berzelius first introduced the chemical symbols, and in 1815 published a classification of minerals based upon chemical composition. In 1841 Rammelsberg wrote the first edition of his excellent "Mineralchemie," and in 1852 Gustav Rose published his Crystallo-chemical System. In this country chemical principles in classification of minerals have been very ably and successfully applied by Prof. Dana, whose present "System of Mineralogy" ranks among the highest. Dana makes the acid constituent the basis of his classification.

Hauy recognizes four classes:

I. *Substances containing acid*—
  Lime, Baryta, &c.

II. *Earthy* substances—
  Quartz, Zircon, &c.

III. *Nonmetallic inflammable* substances—
Sulphur, Diamond, Coal, &c.
IV. *Metallic* substances—
Gold, Silver, &c.

Werner also distinguishes four classes:
I. *Earthy* substances—
Diamond, Augite, Garnet, &c.
II. *Saline* substances—
Soda, Saltpetre, &c.
III. *Inflammable* substances—
Coal, Graphite, &c.
IV. *Metallic* substances—
Gold, Silver, Arsenic, &c.

Between these two systems there is practically very little difference, and that of Weiss resembles them greatly in its main outlines. He decides upon seven orders:
I. *Oxydic* rocks, or Silicates;
II. *Saline rocks;*
III. *Saline ores;*
IV. *Native metals;*
V. *Oxydic ores;*
VI. *Sulphurets* and *sulphides* of *metals;*
VII. *Inflammabilia;*

More arbitrary, however, is the system of Mohs, accepting three classes:
I. *Gases*, water, acids, salts (Soda, Nitre, &c.).
II. *Haloids* (Gypsum, Fluorite, &c. &c.).
III *Gums* and *coal.*

The chemical system as proposed by Berzelius contained two groups with four orders:
A. Oxygen.
I. *Oxygen.*
B. Inflammable substances.
I. *Metalloids,*
II. *Electro-negative* metals,
III. *Electro-positive* metals, closing with Sulphates, Nitrates, and Silicates.

He modified this system, however, and proposed a classification having two orders:
A. Non-oxidized substances.
B. Oxidized substances, arranged according to the electro-negative substance.

With Dana's system everyone is familiar, so that need not further be enumerated here.

In a chemical system we have at hand the means by which a

species or group may be *exactly* defined and limited, while all other systems are based upon more or less arbitrary divisions and assumptions, except those based on crystalline forms; these, however, cannot be properly carried out, as so very large a number of mineral substances show no crystalline structure whatever. In following a chemical system, which may seem the most rational and satisfactory, we dare not lay undue weight upon physical characters. A mineral *species* in any such system must be a single, well-defined group of individuals, and must contain all its subspecies, varieties, and subvarieties, arranged according to rules by which a classification adapted to the nature of minerals can be constructed. It cannot be denied that the physical character of a mineral, even though it has the same composition as some other one, will have some claim upon the attention of a chemical mineralogist, but compared with the composition, it can only be of subordinate value.

Adopting Dana's classification of minerals based upon their chemical constitution, we can divide into:

Divisions,	Subdivisions,
Sections,	Subsections,

and in order to carry out a strict classification, adopt Dana's plan of *Groups*, and then give *Species*, *Subspecies*, *Varieties*, and *Subvarieties*. For instance:

Division:	Oxygen Compounds,
Subdivision:	Ternary Oxygen Compounds,
Section:	Carbonates,
Subsection:	Anhydrous Carbonates,
Group:	Lime Carbonates,
Species:	Calcite,
Subspecies:	Calcite,
Variety:	Arragonite,
Subvariety:	Sprudelstein.

The section "Carbonates" comes under the common general formula, $KO\ CO_2 + (HO)$, and the anhydrous carbonates have $KO\ CO_2$. The name of the *Group* will always be denoted by the name of the basic element, with which for instance carbonic acid is combined, and the simple combination of 1 KO with 1 CO_2 would in this case constitute a species. The specific KO, however, may be replaced in part by some other, and the composition of the mineral will to some extent be changed, whereby a subspecies would be characterized. It is necessary to pay due attention to the differences regarding physical characters within the limits of a species, and as a chemical mineralogist cannot recognize a species created upon physical distinctions alone, and his subspecies denote *chemical* changes, the mineral thus differing will become a variety.

As Dana says (Preface to 4th ed., 1854), "The mind uneducated in science may revolt at seeing a metallic mineral, as Ga-

lena, side by side with one of unmetallic lustre, as Blende," so may an arrangement of this kind seem arbitrary and artificial, but in a science like mineralogy, where we have no generic and specific characters to guide us in the same way as in zoology, and where a classification based upon exterior characters is utterly impossible, we are forced to take the aid of chemistry, and thereby regulate our systems, unless we wish to follow a so-called "natural system," where the arrangement of groups and species is based upon characters of less stability. By all mineralogists of the present day, the importance of chemistry for their studies is freely admitted, and however they may be opposed to any chemical system, in case of a dispute regarding the validity of some species, they are generally obliged to resort to analysis, and in most cases such questions are ultimately decided by the results of such analysis.

As long as "natural systems" are allowed to expand and new ones are created, there can be promised from them no satisfactory classification of minerals giving at the same time due attention to the physical and chemical characters. By adopting a system, however, based upon our present knowledge of the composition of minerals, we are enabled to look forward to some definite arrangement by which *all* the features of a mineral will receive the necessary attention, and only those that are unchangeable and truly characteristic, will be employed in discriminating.

Some systems profess to be arranged in such a manner as to give the *student* more facilities in acquiring the knowledge he seeks. These systems are mostly based upon an arrangement of groups giving "natural" classes. But again to quote Dana: "the distinction of 'useful' and 'useless,' or of 'ores' and 'stones,' although bearing on 'economy,' is not science."

Having given some of the reasons why a chemical system would be preferable to any other, it may be well to propose strict definitions of the various sections, etc., into which the minerals would be divided.

Dana's excellent system has been here adopted as a basis; in the application of the terms Section, Group, and Species, some changes are proposed; Subspecies, Variety, and Subvariety are defined.

A *section*, or divided *subsection*, will have a common general formula.

One or more species and subspecies having this same common general formula and KO alike, will then form a *group*.

Any metal or metalloid substance, occurring *as such* in the mineral kingdom, or the representative of the simple general formula denoting the nature and ratio of combination of any class or subclass, will form a *species*.

In most of the species a larger or smaller part of their basic element is replaced by some other, often even by several, and

such a change in composition, if in certain and stable proportions, not merely an accidental admixture, will demonstrate a *sub-species.*

As a large number of minerals have the same chemical composition, but entirely different physical qualities, it is necessary to regard them as *varieties.* For the chemical mineralogist these differing physical qualities cannot be of sufficient importance to warrant his defining them as species or even subspecies.

Subvarieties will differ from the varieties and subspecies to which they belong in presenting local alterations and replacements of constituents by others in such a manner that they must be regarded merely as accidental admixtures; and subvarieties will be further distinguished by some peculiar method of formation.

In some cases these definitions may not seem to be quite sufficient in determining the position of a mineral. *Dolomite* $\dot{Ca}\,\ddot{C} + \dot{Mg}\,\ddot{C}$ might seemingly be placed under the species Magnesite almost as well as under Calcite, but in any case of this kind we have the physical characters to resort to, and upon investigation find that Dolomite in crystalline form, gravity, etc., more closely resembles Calcite than Magnesite, therefore give it the position as a subspecies of Calcite.

The arrangement of *species* within a *class* or *subclass* ought not to be arbitrary in a strict system, and enumerating them according to their specific gravity of their K, beginning with the lowest figure, might prove satisfactory.

Within the *groups* the species and subspecies must be arranged according to their composition. As the representative of the simple general formula of a class or subclass is defined as species, and the subspecies have part of the K of the species replaced by other substances, they must follow in such order that the one with most varied replacements will form the last member.

The class of Carbonates among the minerals is one of the most generally known, and I have selected the subclass *anhydrous carbonates* for an arrangement based upon the principles above elaborated.

General Formula of Subclass: Anhydrous Carbonates $= \dot{K}\ddot{C}$.

6.	(3.)	2.72.	$\dot{Ca}\,\ddot{C}$	(Calcite.
6.		2.85.	$\dot{Ca}\,\ddot{C} + \dot{Mg}\,\ddot{C}$	(Dolomite.
6.		2.98.	$\dot{Ca}\,\ddot{C} + (\dot{Mg}\,\dot{Fe}\,\dot{Mn})\,\ddot{C}$	(Ankerite.
6.		3.04.	$\dot{Mg}\,\ddot{C}$	(Magnesite.
6.		3.33.	$\dot{Mg}\,\ddot{C} + \frac{1}{2}\,\dot{Fe}\,\ddot{C}$	(Mesitite.
6.		3.41.	$\dot{Mg}\,\ddot{C} + \dot{Fe}\,\ddot{C}$	(Pistomesite.
3.		4.29.	$\dot{Ba}\,\ddot{C}$	(Witherite.
4.	(3.)	3.64.	$\dot{Ba}\,\ddot{C} + \dot{Ca}\,\ddot{C}$	(Barytocalcite.
3.		3.65.	$\dot{Sr}\,\ddot{C}$	(Strontianite.
6.		4.42.	$\dot{Zn}\,\ddot{C}$	(Smithsonite.

6.		$\dot{Zn}\ddot{C} + \dot{Cu}\ddot{C}$?	(Herrite.
6.		$\dot{Zn}\ddot{C} + \dot{Fe}\ddot{C}$	(Capnite.
6.	3.55.	$\dot{Mn}\ddot{C}$	(Rhodochrosite.
3.	3.03.	$\dot{Mn}\ddot{C} + \frac{1}{2}(\dot{Ca}\dot{Mg})\ddot{C}$	(Manganocalc.
6.	3.80.	$\dot{Fe}\ddot{C}$	(Siderite.
6.	3.41.	$\dot{Fe}\ddot{C} + \dot{Ca}\ddot{C}$	(Siderodote.
3.	6.46.	$\dot{Pb}\ddot{C}$	(Cerussite.
		$\dot{Pb}\ddot{C} + \frac{1}{3}\dot{Zn}\ddot{C}$	(Tglesiasite.
2.	6.31.	$\dot{Pb}\ddot{C} + Pb\,Ce$	(Phosgenite.

Appendix.

a.	4.78.	$\dot{La}\ddot{C} + \frac{1}{3}Ce\,(FO)_3$	(Kishtimite.
b.	4.50.	$(\dot{La}\dot{Ce}\dot{Di})\ddot{C} + \frac{1}{3}(\dot{Ca}\dot{Ce})F$	(Parisite.

Although the gravity of Lanthan is 0.833, it became necessary to place the Lanthan minerals as an appendix, because $\dot{La}\ddot{C}$, which would be the species, is not known at present, and therefore their position with reference to the other anhydrous carbonates is not quite clear.

It will readily be seen that we obtain by this arrangement groups very similar (*natural*) both in their chemical composition and their physical characters

In acknowledging only such as species that simply fill the general formula of some class or subclass, and regarding every mineral of the same chemical character, but with various replacements of RO, as a subspecies, many minerals heretofore regarded as species will only obtain the rank of subspecies or of varieties.

Thus, *Aragonite*, with the same composition as Calcite, but differing in crystalline form, gravity, hardness, etc., can only be regarded as a variety of the species and subspecies Calcite. *Graphite*, chemically identified with Diamond, although physically differing, will in a strict system receive the place of a variety. *Metacinnabarite*, chemically Cinnabarite, differing, however, in structure, gravity, color, etc., can be nothing but a variety of the latter mineral.

In retaining the one name for the species or subspecies, the right of priority will be observed. Metacinnabarite was named in 1872, while "Cinnabarite" has been in use already for a very long time.

In proposing the above rules for classifying minerals, one of my chief objects is to assume a basis for a more rigorous distinction of the terms species and varieties and those subordinated to them, and although this to a certain extent may seem to be going too far, in partly giving up the characteristic individuality of a mineral, a classification of this kind may claim to be a more logical one than many others, and in a science like mineralogy this may perhaps be one of the chief requisites.

45TH MEETING. APRIL 12, 1873.

The President in the Chair.

Mr. J. E. HILGARD made a communication entitled:

AN INQUIRY INTO THE LAWS OF PROBABILITY.

46TH MEETING. APRIL 26, 1873.

The President in the Chair.

Mr. G. K. GILBERT presented a communication

ON THE GLACIAL EPOCH IN UTAH AND NEVADA.

(ABSTRACT.)

Ancient glacial moraines and cognate phenomena are known upon the Sierra Nevada as far south as N. lat. 36° 30′, and on the Rocky Mountains to nearly the same latitude; but they are confined to high mountain valleys. The glaciers that produced them did not extend to the lower valleys. There is evidence in the perfect preservation of quaternary and pliocene deposits throughout the intervening "Great Basin," that it was subjected to no general glaciation during the Glacial epoch; but glaciers existed on the flanks of the highest mountains, and to the list of these the Engineer explorations in 1872 made several additions. The new localities are: White's Peak, Schell Creek range, Nevada, N. lat. 39° 15′; Wheeler's Peak, Snake range, Nevada, N lat. 39°; Belknap Peak, near Beaver, Utah, N. lat. 38° 25′; and Fish Lake, Utah, N. lat 38° 30′. The phenomena consist of moraines and moraine lakes, and are found no lower than 8000 feet above the sea level. These points are believed to be the most southerly at which, in these latitudes, such traces can be found

The undrained valleys of the "Great Basin" have been filled, at a recent geological period, by large lakes, of which Great Salt, Sevier, Humboldt, Owens, etc. are the remnants. Mr. Howell and the writer have studied during the past summer the beaches and deposits of the lake that covered the Sevier and Great Salt Lake deserts, and mapped a large portion of its outline. It had an extreme depth of one thousand feet, and an average depth of four hundred. Its length was three hundred and fifty miles, and its area nearly equalled that of Lake Huron. The mountain ridges, with which the region abounds, studded its expanse with islands, and complicated its shore with peninsulas. Its water

was fresh, and there is abundant reason to believe it overflowed its basin, sending its surplus to the ocean.

This lake is referred to the Glacial epoch by reason of the following coincidence: Upon the slopes which exhibit its sediments there are also gravels, brought down from the mountains by running water and spread by the same without thorough sorting. A small portion of these gravels overlie the lacustrine beds, but the great mass underlies them. The sequence, gravel, marl, gravel, shows that the lake epoch was transient, being preceded as well as followed by a period when, as now, the climate was too arid to maintain a broad water surface. It was a climatal episode characterized by increased humidity, or lower temperature, or both. The Glacial epoch was a climatal episode of the same general character, and occurred in the same general geological period—that immediately antecedent to modern time. There is the same reason for supposing the two coincident in time, that there is for correlating the European with the American Glacial epoch.

At that time the Atlantic slope and the region of the "Great Basin" were contrasted in climate, just as now. The general causes that covered the humid east with a mantle of ice, sufficed in the arid west only to flood the valleys with fresh water and send a few ice streams down the highest mountain gorges.

Prof. J. R. EASTMAN read a paper on

THE FREQUENCY OF THE OCCURRENCE OF THE ZERO AND THE NINE DIGITS IN THE TENTHS OF SECONDS AS OBTAINED FROM THE CHRONOGRAPHIC RECORD OF TRANSIT OBSERVATIONS.

(ABSTRACT.)

A partial examination of the records of observations with the Transit Circle at the Naval Observatory was made in 1870 in order to determine whether, in reading the recorded transits from the chronograph sheets, there was a tendency to record any particular figures in the observing book.

This examination tended to confirm my belief that no such bias existed, but I was unable to complete the investigation until the spring of 1873.

In the latter investigation an equal nnmber of records were used from each of two different chronographs. On each chronograph the clock closes the circuit each second. One which I shall refer to as chronograph A, employs only one pen, and the speed is regulated by a friction governor. The length of a second's interval is 0.426 inch. The other chronograph, which I shall call chronograph B, uses only one pen, and the length of the second's interval is one-third of an inch.

This instrument is known as the Hippe chronograph, the gov-

ernor being a vibrating spring. The observations of 510 stars over nine transit threads were examined from each chronograph. These stars were observed in the last six months of 1872.

The following record of an observation of α Canis Minoris will illustrate the manner of recording the work in the observing book:—

	$40^{s}.2$
	42 .7
	44 .3
	50 .5
$7^{h}\ 32^{m}$	52 .5
	54 .5
	0 .8
	2 .3
	4 .8

These records are read from the sheets by means of scales carefully graduated to tenths of the second's interval.

The number of times that each figure from 0 to 9, inclusive, occurred in the tenths of seconds in the one thousand and twenty observations, were carefully counted, and the results are shown in the following table:—

From Chronograph A.

0	1	2	3	4	5	6	7	8	9
729	258	438	434	450	494	459	446	432	450

From Chronograph B.

0	1	2	3	4	5	6	7	8	9
962	110	349	436	480	464	463	431	471	424

A slight inspection of the results from chronograph A shows a striking difference between the 0s and 1s, and a similar difference occurs in the work from chronograph B between the 0s, 1s, 2s, and 9s.

Omitting those numbers and using the results from both chronographs for the figures 3 to 8, inclusive, it appears that the probable error of a single sum is less than $\frac{1}{30}$ of the mean. As this probable error decreases with the increase of the number of observations examined, it seems perfectly safe to conclude that there is no tendency in my work to record any particular figures.

The discrepancies in the results for 0, 1, 2, and 9, can be explained by the action of the chronograph pen and by the peculiarities of the two chronographs.

If the armature of the magnet which carries the pen be so adjusted as to make the length of the clock-mark more than $\frac{1}{20}$ of the interval corresponding to one second, all marks of the observer's key which might happen to fall in the time while the clock keeps the circuit closed will be at least uncertain, and can

only be read as zero. It is evident, therefore, that 0.35 of the zeros from chronograph A should be read as ones.

As the length of the interval is reduced by using a smaller cylinder, the adjustment of the length of the clock-mark to $\frac{1}{20}$ of a second becomes more difficult, and, accordingly, we find not only the *ones*, but the *twos* and *nines* affected in the work from chronograph B.

Representing the number of zeros in a series of observations of the same object by N, and the number of complete observations over nine threads by n, the probable correction to be applied to the mean result of observations taken from chronograph A would be $\frac{N \times 0.35}{9n}$,

and from chronograph B $\frac{N(0.34 + 2 \times 0.11 - 0.04)}{9n} = \frac{N \times 0.52}{9n}$.

The adjustment of the clock-mark to less than $\frac{1}{20}$ of a second for chronograph B requires a strong and constant battery power, which can only be secured by such assiduous watching that it becomes nearly impracticable in nearly all routine astronomical work. This difficulty can be reduced one-half by allowing the clock to mark only once in two seconds; but the only complete solution of the problem seems to be to increase the length of the interval.

Mr. Joseph Henry, in the absence of Mr. C. S. Peirce, gave the third of his lectures

ON ATMOSPHERIC ELECTRICITY.

47th Meeting. May 10, 1873.

Vice-President J. E. Hilgard in the Chair.

The Chair announced that in respect to the memory of the late Chief Justice, Salmon P. Chase, the general committee had determined that this meeting of the Society should be adjourned for one week. Dr Peter Parker then presented, with some appropriate remarks, the following Resolutions, which were adopted:—

Whereas, since the last meeting of the Philosophical Society of Washington, Salmon Portland Chase, Chief Justice of the United States, an esteemed and honored member of this Society, has departed this life full of years and full of honors, no more to meet his associates here :

Resolved, That in this event we recognize with profound emotion the dispensation of Divine Providence, which has unexpectedly deprived the Society of one of its most distinguished members, and the nation of a jurist, patriot, statesman, philanthropist, and Christian; whose name, when the course of time is run and the final history of the American Republic shall have been written, shall be classed with those of Washington and Lincoln, and of all those eminent men who by Providence have been raised up for special crises of the Republic, and endowed with extraordinary qualifications for national and extreme emergencies, and as such his memory will be cherished by this Society.

Resolved, That as a token of our deep sense of the solemn event, and of our great esteem for our departed associate, the Society now adjourn.

Resolved, That a copy of these Resolutions be conveyed to the bereaved and deeply afflicted relatives.

48TH MEETING. MAY 17, 1873.

The President in the Chair.

Mr. J. J. WOODWARD read a portion of a letter from Dr. B. A. Gould, Director of the National Observatory at Cordoba, giving an account of the progress of his astronomical work.

Mr. C. S. PEIRCE made a communication

ON LOGICAL ALGEBRA.

(*This paper is published in full in the Memoirs of the American Academy of Arts and Sciences.*)

Mr. A. HALL read a paper

ON THE RECTILINEAR MOTION OF A PARTICLE TOWARDS AN ATTRACTING CENTRE.

(*This communication is published in the "Messenger of Mathematics," Dec.* 1873.)

Mr. G. K. GILBERT read a paper

ON THE USE OF THE CAÑONS OF THE COLORADO FOR WEIGHING THE EARTH.

49TH MEETING. MAY 24, 1873.

The President in the Chair.

Lieut. C. E. DUTTON read a memoir

ON GEOLOGICAL TIME.

50TH MEETING. JUNE 7, 1873.

The President in the Chair.

Mr. S. NEWCOMB made a communication

ON THE MECHANICAL REPRESENTATION OF A PROBLEM IN LEAST SQUARES.

(*This paper is published in full in Monthly Notices of the Royal Astronomical Society, November*, 1873.)

Mr. H. H. BATES read a paper

ON THE MOTION OF A PARTICLE TOWARDS AN ATTRACTING CENTRE.

Mr. R. KEITH read a paper

ON THE NATURE OF THE FORCE OF GRAVITATION.

51ST MEETING. JUNE 21, 1873.

The President in the Chair.

Mr. J. E. HILGARD made some remarks

ON THE AIR THERMOMETER OF PROF. JOLLY,

exhibiting also one of those instruments. The same gentleman also made the second communication

ON THE RECENT DETERMINATION OF THE LONGITUDE BETWEEN PARIS AND GREENWICH.

Mr. J. J. WOODWARD then exhibited, with appropriate remarks, the spectra of certain metals and gases. The apparatus used was a four-prism spectroscope by Browning, with a six-inch induction coil by Ritchie and a condenser by Ladd. He had also upon the table several smaller spectroscopes, with which the spectra of various chlorides were shown.

52D MEETING. OCTOBER 4, 1873.

The President in the Chair.

The President alluded to the loss the Society had just sustained by the death of Prof. G. C. Schaeffer, a member of the Society and one of the oldest members of the original club. The following resolutions were then adopted, as reported by Mr. W. B. Taylor from the general committee :—

WHEREAS, we have heard with profound regret of the decease of our esteemed friend and colleague, Professor GEORGE C. SCHAEFFER—

Resolved, That while we have to deplore the death of an associate personally endeared to us, we feel that our Society has sustained the loss of one who was for many years earnestly devoted to the cultivation and promotion of general science.

Resolved, That we hereby express our high appreciation of the eminent abilities of our departed friend, and of the unusually wide and varied range of his scientific attainments.

Resolved, That we tender our sincere sympathy to his family in their bereavement, and that the Secretary of the Society be directed to transmit to them a copy of these resolutions.

After further remarks by Mr. J. E. HILGARD the Society then adjourned.

53D MEETING. OCTOBER 18, 1873.

The President in the Chair.

Mr. J. E. HILGARD gave an account of

RECENT EXPERIMENTAL RESEARCHES IN ACOUSTICS, BY PROF. A. M. MAYER.

Lieut. C. E. DUTTON made a communication

ON MALLET'S THEORY OF THE FORMATION OF THE PHYSICAL FEATURES OF THE EARTH.

Mr. JOSEPH HENRY gave an account of his

EXPERIMENTS ON FOG SIGNALS DURING THE PAST SUMMER.

54TH MEETING. NOVEMBER 1, 1873.

The President in the Chair.

This being the annual business meeting for the election of officers, the order of proceedings for the evening was announced by the President. On motion of General SHERMAN, the rules were suspended and Mr. JOSEPH HENRY was re-elected President by unanimous consent. The following gentlemen were elected Vice-Presidents: J. E. HILGARD, M. C. MEIGS, J. K. BARNES. As Secretaries, THEO. GILL and C. ABBE were elected. As members at large of the general committee, there were elected the following gentlemen:—

S. F. BAIRD,	N. S. LINCOLN,
J. H. C. COFFIN,	O. M. POE,
E. B. ELLIOTT,	S. NEWCOMB,
A. HALL,	J. C. WELLING,

J. J. WOODWARD.

Mr. B. ALVORD called the attention of the meeting to an announcement in a San Francisco newspaper that Mr. James Lick, of San Francisco, intended to found an astronomical observatory on the Sierra Nevada.

55TH MEETING. NOVEMBER 15, 1873.

The President in the Chair.

Mr. JOSEPH HENRY read a letter from Prof. Tyndall, of London, relating to fog signals, and a second from Mr. James Lick, of San Francisco, respecting the establishment of an observatory and the construction of a large telescope.

Mr. E. B. ELLIOTT called the attention of the Society to the change in the legal value of the dollar just adopted in England.

Mr. W. H. ELLIOTT then made a verbal communication

ON THE HABITS OF THE FUR-BEARING SEALS OF THE ISLANDS OF ST. PAUL AND ST GEORGE, BEHRING SEA,

illustrating his remarks by numerous original drawings.

(This communication will be found printed as a portion of a Report to the Secretary of the Treasury.)

56TH MEETING. NOVEMBER 19, 1873.

The President in the Chair.

The President announced that the meeting was called at this date especially to receive Mr. Alvan Clark, of Cambridge, Mass. Mr. Clark then gave

A DETAILED ACCOUNT OF THE CONSTRUCTION OF THE LENSES AND OTHER INTERESTING PORTIONS OF THE LARGE TELESCOPE NOW ESTABLISHED AT THE NAVAL OBSERVATORY.

The thanks of the Society were voted to Mr. Clark for his communication.

57TH MEETING. NOVEMBER 26, 1873.

The President in the Chair.

The President announced that the meeting had been called this evening especially to listen to a communication from Dr. Bessels of the Polaris Exploring Expedition. After further introductory remarks by the Chair, Dr. Bessels read a paper

ON SOME OF THE RESULTS OF THE POLARIS NORTH POLE EXPEDITION.

(*This communication will form a part of the detailed Report of Dr. Bessels to the National Academy of Sciences.*)

58TH MEETING. DECEMBER 6, 1873.

The President in the Chair.

Mr. J. E. HILGARD read a communication

ON THE DETERMINATION OF THE PERSONAL ERRORS IN THE OBSERVATION OF ASTRONOMICAL TRANSITS,

illustrating his remarks by the exhibition of apparatus recently constructed for the use of the Coast Survey.

Mr. J. S. BILLINGS presented a memoir

ON THE COLLECTION OF A LARGE LIBRARY,

having especial reference to the medical library of the Surgeon-General's Office.

(ABSTRACT.)

The Library of the Surgeon-General's Office may be considered as the Medical Section of the National Library, and is catalogued and conducted upon the same plan as the Congressional Library. It contains about 25,500 volumes and 18,000 pamphlets. One of the most valuable sections of a medical library is that of the Journals and Transactions. This library contains about 7000 volumes of Journals, of which about 1850 volumes are American, 2700 German, 1250 French, 1050 English, and the remainder Russian, Italian, and Spanish. The first American medical journal began in 1798, and since that date 451 medical journals have been commenced in this country.

Attention was then called to some of the old and rare books of the collection, including a MS. of the Lilium Medicinæ of Bernard de Gordon, dated 1349; copies of the works of Gersdorff and Braunschweig, and a number of books dated prior to 1500.

The Library contains a large number of medical theses and dissertations, and the best mode of binding these was referred to. They will be bound by Universities where complete sets can be obtained, otherwise by subjects. Pamphlets of especial rarity or value are bound single, but on the ground of economy, convenience, and to prevent loss, most of them will be bound in volumes by subjects. An Alphabetical Catalogue of Authors has just been printed, and the preparation of a subject catalogue is to be commenced at once.

59TH MEETING. DECEMBER 20, 1873.

The President in the Chair.

The President announced the death of Professor Louis Agassiz, and on motion of Hon. PETER PARKER, a committee was appointed to draft appropriate resolutions, of which the President was, on motion, made the chairman.

Mr. J. J. WOODWARD communicated portions of a letter from Dr. Jackson, of Boston, detailing some of the results of the autopsy of Professor Agassiz.

The same gentleman then made a short communication

ON MICROMETRIC WRITING ON GLASS,

illustrating his remarks by photographs of Noberts' lines, and of the microscopic writing of Mr. Webb, of London.

Mr. A. HALL read a paper

ON COMETS AND METEORS.

(*This communication is published in full in "The Analyst," vol. i. pp.* 17–24, *Des Moines, Iowa, Feb.* 1874.)

60TH MEETING. JANUARY 3, 1874.

The President in the Chair.

Hon. PETER PARKER read a short note on the meteor of Dec. 24th, 1873, communicating the details of some observations made by himself.

Following this communication there were given the experiences of a number of observers who saw this meteor, and, on motion of Dr. Parker, the President appointed a committee to collect further information.

Mr. J. J. WOODWARD read a letter from Mr. Ramsey, of Nova Scotia,

ON THE TIDES OF THE BAY OF FUNDY.

Mr. G A. OTIS gave

A DESCRIPTION OF A NEW SPIROMETER,

illustrating his remarks by means of the instrument itself.

Mr. C. S. PEIRCE communicated a note

ON QUATERNIONS, AS DEVELOPED FROM THE GENERAL THEORY OF THE LOGIC OF RELATIVES.

61ST MEETING. JANUARY 17, 1874.

The President in the Chair.

The President, as chairman of the committee, offered the following resolutions, which were adopted, relative to the death of Prof. Louis Agassiz:—

Resolved, That the members of the Washington Philosophical Society have heard with profound regret of the death of Professor Louis Agassiz.

Resolved, That in the death of this eminent savant the world has lost an efficient contributor to the store of human knowledge, the results of whose labors must ever occupy a conspicuous place in the history of Science.

Resolved, That in his adopted country he has nobly vindicated the claims of scientific investigation to high popular appreciation and to liberal public patronage.

Resolved, That as an instructor he has introduced methods of study and directed the attention of students in natural history to fields of research far superior to those which were cultivated previous to his coming among us.

Resolved, That his ardent sympathy and genial manners endeared him to thousands of the inhabitants of this country, who regard his death as a personal as well as a public calamity.

Resolved, That the members of this Society deeply sympathize with the family of Professor Agassiz in the great loss they have sustained, and that a copy of these resolutions be transmitted to them by the President.

By request of the President, the Secretary read a letter from Mr. Benjamin Hallowell, of Sandy Springs, Md., relating to the meteor of Christmas eve.

Mr. A. HALL, in the absence of the author, then read a memoir by Mr. E. S. HOLDEN,

ON THE ADOPTED VALUE OF THE SUN'S APPARENT DIAMETER.

(*See the Appendix to this Bulletin.*)

62D MEETING. JANUARY 31, 1874.

The President in the Chair.

Mr. F. M. ENDLICH made a communication

ON ELECTRICAL PHENOMENA IN THE ROCKY MOUNTAINS.

Mr. C. E. DUTTON made a communication

ON RECENT IMPROVEMENTS IN THE ECONOMY OF FUEL.

Mr. J. W. POWELL read a paper

ON THE MYTHOLOGY OF THE NUMAS.

(This paper will be published in full in a forthcoming Report to the Secretary of the Smithsonian Institution, on the Survey of the Colorado River.)

63D MEETING. FEBRUARY 14, 1874.

The President in the Chair.

The President announced the names of gentlemen recently elected members of the Society.

Mr. WM. HARKNESS read an abstract of a paper

ON THE DISTRIBUTION OF TEMPERATURE OVER THE SURFACE OF THE GLOBE.

The author presented to the Society by means of a series of diagrams projected on a screen, fac-similes of tables and curves elaborated by himself, and which embodied the results obtained by the study of the annual variations of temperature at over nine hundred stations, embracing about 7000 years of observations.

(This paper will be published in full by the Smithsonian Institution.)

Mr. THEODORE GILL made a communication

ON THE PRIMATES AND THEIR RELATIONS TO MAN.

64TH MEETING. FEBRUARY 28, 1874.

Vice-President W. B. TAYLOR in the Chair.

Mr. ELLIOTT COUES made some remarks

ON THE STRUCTURE AND HOMOLOGIES OF THE LIMBS, ESPECIALLY IN AVES.

Following this communication Mr. THEODORE GILL made some

remarks thereon, and dwelt particularly on the homologies of the limbs of the lower vertebrates.

Mr. L. D. Gale made a communication

ON THE CAUSE AND REMEDY OF THE POTATO ROT.

The author maintained the so-called "chemical theory" of the origin of this disease, and adduced in corroboration experiments made during the past ten years in Long Island and New Jersey.

Remarks followed by various persons present, especially by Prof. Brewer, of Yale College; the latter gave a brief synopsis of the many theories that have been broached in reference to this phenomenon, and showed the unsatisfactory nature of conclusions deduced from any partial investigation.

65th Meeting. March 14, 1874.

The President in the Chair.

Mr. L. D. Gale offered some remarks in explanation of his communication at the previous meeting in reference to the potato disease.

Mr. C. S. Peirce made a communication

ON VARIOUS HYPOTHESES IN REFERENCE TO SPACE.

Mr. J. M. Toner illustrated, by means of a map,

A METHOD OF DESCRIBING AND LOCATING WITH EASE THE APPROXIMATE POSITIONS OF GEOGRAPHICAL REGIONS.

Mr. Joseph Henry made a communication

ON A METHOD OF DEVELOPING MAGNETISM IN BARS OF STEEL.

66th Meeting. March 28, 1874.

The President in the Chair.

Mr. C. E. Dutton made a communication

ON RECENT IMPROVEMENTS IN THE MANUFACTURE OF STEEL.

Prof. C. G. FORSHEY, of Louisiana, made a communication

ON THE ALLUVIAL BASIN OF THE MISSISSIPPI RIVER STYLED THE DELTA.

(*See the Appendix to this Bulletin.*)

67TH MEETING. APRIL 11, 1874.

The President in the Chair.

At the request of Mr. J. E. HILGARD, Mr. C. E. DUTTON, in continuation of his previous communication, gave a synopsis of his views

ON THE CHEMISTRY OF THE BESSEMER PROCESS.

Mr. E. FOOTE made some remarks

ON SOME CAUSES THAT PRODUCE RAIN.

Mr. F. M. ENDLICH read a paper

ON TWO BRICKS FROM THE GREAT WALL OF CHINA.

The same gentleman made a second communication

ON SPECIMENS OF METEORIC IRON FROM CHIHUAHUA, MEXICO, AND THE STRUCTURE OF METEORITES IN GENERAL.

In connection with the preceding paper Mr. JOSEPH HENRY made some remarks in reference to the recent observations of Prof. Newton, of Yale College,

ON METEOR TRAINS, AND THE UPPER ATMOSPHERIC CURRENTS.

In answer to inquiries of Mr. J. E. HILGARD, Mr. CLEVELAND ABBE, as Secretary of the Committee appointed to collect observations on the meteor of December 24th, 1873, made a brief statement of the condition of their work.

In the course of some remarks on the reported earthquake phenomena in North Carolina, the desire was expressed that a com-

mittee should be appointed to collect information in reference thereto.

Mr. J. W. POWELL desired to state his conviction that Arizona and the southern portion of Nevada had within quite recent times been the seat of considerable volcanic and seismic activity.

68TH MEETING. APRIL 25, 1874.

The President in the Chair.

Mr. THEODORE GILL made a communication

ON THE STRUCTURE AND SHAPE OF PALÆOTHERIUM.

(ABSTRACT.)

Mr. Gill referred to the recent discovery of a very complete skeleton of a species of Palæotherium in a plaster quarry near Paris, with the bones in place, and indicating its natural attitude. This recalled the form of the Llama, and the speaker reminded the Society that on a previous occasion, in a communication on the Tapiridæ, he had contended that there were no near affinities between the Tapirids and Palæotheriids, and that one of the latter, judging from the separate bones, had been quite erroneohsly restored by Cuvier under the prejudice that it was like the Tapirids. He then contended that the form of Palæotherium was light and slender, and the neck long, and that the absence of any muscular scars like those of Tapirids, as well as the relations of the bones, indicates that it had no proboscis. While the discovery of the new skeleton has now authoritatively dissipated the idea of the form of the animal as delineated by Cuvier, the comments thereon indicate that the idea as to the proboscis is still prevalent. The speaker gave in full his reasons for denying the existence of such a proboscis in Palæotheriids, as well as in Macranchenia and the Dinocerata.

Mr. JOSEPH HENRY made some remarks

ON GIFFARD'S INJECTOR.

Mr. CLEVELAND ABBE made a verbal communication

ON THE LAWS GOVERNING THE MOVEMENT OF STORM CENTRES.

(ABSTRACT.)

Mr. Abbe called attention to a recent memoir by Professor Loomis, of Yale College, on the storms of the United States during the years 1872 and 1873. He stated that the generalizations announced by Professor Loomis constituted a most important contribution to our knowledge of this subject, and that the laws mathematically deduced from mechanical principles by Mr. Wm. Ferrel in his memoir on Winds and Currents, were in every instance corroborated by observations in both Europe and America. He stated that the law announced by himself at Cincinnati in 1869 remained abundantly confirmed by daily experience, and might be concisely expressed as follows: a storm centre moves toward the region where a given barometric or other condition produces the greatest precipitation of aqueous vapor, or in which the latter is most rapidly taking place. In explanation of this law he added that when we take into consideration all the causes that contribute to produce the precipitation of vapor (whether in the form of haze, fog, cloud, rain, snow, or hail), we are able to account with great accuracy for the direction and velocity of movement of areas of low barometer.

Atmospheric precipitation is produced by cooling the air, and a fall of temperature in any gas is the consequence either of the radiation of heat or of the absorption of heat in performing internal work.

The mechanical absorption of heat is an important feature whenever masses of air are elevated and allowed to expand. This occurs under the following circumstances: (1) whenever strong winds blow, and in consequence of the inertia of the air and the friction on the earth's surface, produce vertical currents; (2) in consequence of winds being pushed up an inclined plane, such as the great plains of the Mississippi Basin, or the ascents on the east and west sides respectively of the Appalachian range, and the Sierra Nevada; (3) in consequence of the elevation of masses of warm air above the masses of cold air, which latter flow, for example, from the extreme N. W. southward to the Gulf of Mexico, down the gentle grade of the Missouri and Mississippi Valleys (under this head are included the formation of the local thunder storms).

The radiation of heat may take place either (1) outward into space whenever the air above is dry; or, (2) downward either to the cold earth, or to masses of cold air that have underrun and uplifted warmer layers: radiation into space is especially effective after a mass of moist air has been thus uplifted.

The radiation of heat and the visible precipitation of vapor are remarkably counteracted whenever extensive fires prevail, by the presence in the atmosphere of very minute particles of carbon or of vegetable ashes, which have the property of attracting about themselves quite dense atmospheres of aqueous vapor precisely as oxygen is occluded by finely divided platinum.

The direction in which a storm moves being thus dependent to a great degree on the precipitation of moisture, it becomes important to know the location of the sources of atmospheric vapor, especially the presence of regions of snow, forests, swamps, etc.; and explanations were given of certain abnormal storm paths quoted by Professor Loomis.

Especial objection was urged against the idea that high westerly currents carried the storms of America eastward.

Mr. F. M. ENDLICH read a paper

ON THE OCCURRENCE OF PURE TELLURIUM IN CERTAIN GOLD MINES OF COLORADO.

Mr. ASAPH HALL offered some remarks

ON THE METHOD ADOPTED IN WRITING THE INTERNATIONAL SCIENTIFIC TELEGRAMS.

Mr. HALL recommended the use of Littrow's system.

69TH MEETING. MAY 9, 1874.

The President in the Chair.

Mr. BENJAMIN ALVORD read a paper

ON THE RECENT EARTHQUAKES IN NORTH CAROLINA.

According to Professor Warren Du Pré, of Wofford College, Spartanburg, S. C., the earthquake phenomena in North Carolina commenced on February 10, 1874, and continued until the date of his visit to the scene of the disturbances. They appeared within a region of the Blue Ridge about twelve miles north from Rutherfordton, N. C., and fourteen miles S. E. from Mount Mitchell, the highest peak in the United States east of the Rocky Mountains. They occurred in a space of about twenty-five miles square. The mountain ridge in which they appeared has several peaks, the highest, called Bald, Stone, and Round Mountains, extending from southwest to northeast a distance of ten miles, in the order in which they are named. They are from 3000 to 3500 feet high. They are covered with a dense forest, and composed

of granite, gneiss, and mica-slate rocks. None of the rocks usually called volcanic rocks have been discovered there.

From the 10th of February to the 19th of March, the day of his visit, there had been about seventy-five shocks, some occurring while he was there. The noises began with an explosion like a blast, followed by a rumbling sound lasting only a few seconds: the shocks were simultaneous, or almost so, with the reports, and seemed to follow the direction of the rumbling sound, with the exception that observers near the top of the mountain assert they appear to be under and all around them; that the reports all came from Stone and Bald Mountain ridge; those living on the east side pointing to the west, and those on the west pointing to the east for the direction of the sounds: that the effects were felt five miles on each side of the mountain ridge. In all these convulsions of the mountain the concurrent testimony is, *that the sounds and shocks were either simultaneous or nearly so.*

Following the preceding paper, Mr. J. W. POWELL stated that he desired to repeat a statement previously made by himself, viz., that the generally accepted rule that volcanic action is peculiar to the neighborhood of the sea, rests on very limited data, and that if we consider the geological epoch immediately preceding the present, we find such action well developed in regions far distant from the ocean, as in western America.

Mr. C. E. DUTTON remarked that volcanic action is especially remarkable in localities that have been the scene of the shifting of sedimentary deposits.

Mr. CLEVELAND ABBE called attention to the observations of Professor Niles, at Monson, Mass., as affording possibly a mechanical explanation of the phenomena in North Carolina.

Mr. W. HARKNESS made a communication

ON THE APPARATUS TO BE USED IN THE OBSERVATIONS OF THE APPROACHING TRANSIT OF VENUS.

This paper was illustrated by transparent photographs exhibited by means of the calcium light.

Mr. J. K. GILBERT read a paper

ON A COLD GEYSER OR INTERMITTENT ARTESIAN WELL IN OHIO.

(ABSTRACT.)

Mr. Gilbert described an artesian well that had been bored at Stryker, Williams County, Ohio, to a depth of 860 feet. Water and gas had been met with at several points, and especially a mineral water at 230 feet. There is no continuous outflow of water, but a level is maintained a little below the surface of the ground, and gas (sulphydric acid) constantly escapes. After intervals, normally of about six hours, there is a spasmodic discharge of a large volume of gas, with such force as to carry with it many barrels of water, the eruption lasting ten to twenty minutes. By nearly closing the mouth of the well its discharge can be indefinitely deferred, and by agitating the water with a pole, so as to produce a vertical oscillation, the flow can be hastened. The agency of heat, essential to the best sustained theories of geyser action, is here out of the question, and a mechanical explanation is in order. From the possibility of a vertical vibration of the water is inferred its communication with a reservoir stored with gas; and it is further hypothecated that, from some source, there is a constant accession of gas. When its volume is such that it displaces the water at the point of communication with the well, it begins to escape, and, at once, by its *motion*, clears a larger opening. Rising in the well, its tendency is to lift the water in part, and, by relieving the pressure, enable the elastic gas remaining in the reservoir to expand more rapidly. From the reciprocation of these actions results the paroxysmal discharge.

Mr. F. W. CLARKE made a verbal communication

ON THE ATOMIC VOLUMES OF CRYSTALLIZED AND DOUBLE SALTS.

(*This paper will be published in the American Journal of Science and Arts.*)

70TH MEETING. MAY 23, 1874.

The President in the Chair.

The President offered some remarks on the communication of Mr. Benj. Alvord, made at the previous meeting.

Mr. J. E. HILGARD then introduced Hon. T. L. CLINGMAN, of North Carolina, who made a communication

ON THE EARTHQUAKE PHENOMENA RECENTLY EXPERIENCED IN NORTH CAROLINA.

(ABSTRACT.)

Mr. Clingman stated that the first shock on record occurred in 1811, and similar ones had been experienced every two or three years since that date, in some portion of a region including the six most western counties of North Carolina, all of them immediately east of the main range of the Blue Ridge.

The author had devoted much time to the investigation of this subject, and gave many interesting details relative thereto.

(This communication has been published in the New York Daily Times, July 15, 1874.)

Mr. J. W. POWELL read a memoir

ON THE GENESIS AND DEMONOLOGY OF THE NUMA TRIBE OF INDIANS.

(This paper will be published in full in a forthcoming report of Mr. Powell to the Secretary of the Smithsonian Institution, On the Survey of the Colorado River.)

71ST MEETING. JUNE 6, 1874.

Vice-President J. E. HILGARD in the Chair.

The chairman announced the adoption by the general committee of a resolution ordering that in official records of the Society all titles of members other than "Mr." be hereafter omitted.

Mr. F. W. CLARKE made a communication

ON THE MOLECULAR HEATS OF SIMILAR COMPOUNDS.

(ABSTRACT.)

It is laid down in all text-books that similar compounds have equal atomic or molecular heats. Thus, for instance, the chlorides of the alkaline metals are said to have equal values, the carbonates of the calcium group furnish a similar example, and so on. This is probably true, with certain qualifications. The different bodies must be compared not at the same temperature, but at what are called *corresponding* temperatures. These correspond-

ing temperatures would probably be at equal distances from the melting points, although this is by no means certain. Under ordinary circumstances, in any given series of similar compounds the molecular heat increases slowly with the atomic weight. The two subjoined series, series which are usually taken to illustrate the supposed equality, will exemplify this fact. Each molecular heat is here deduced from the mean of all the reliable determinations of specific heat extant for each substance. The first series is that of the alkaline chlorides.

SUBSTANCE.	MOLECULAR HEATS.
Li Cl	11.62.
Wa Cl	12.56.
K Cl	12.93.
Rb Cl	13.54.

No determinations have yet been made for Cs Cl.
For the carbonates of the calcium groups,

SUBSTANCE.	MOLECULAR HEATS.
$Ca\ CO_3$	21.09.
$Sr\ CO_3$	21.34.
$Ba\ CO_3$	21.49.
$Pb\ CO_3$	21.91.

Comparing some chlorides, bromides, and iodides, we have—

SUBSTANCE.	MOLECULAR HEATS.
K Cl	12.93.
K Br	13 47.
K I	13.60.
$Pb\ Cl_2$	18.85.
$Pb\ Br_2$	19.55.
$Pb\ I_2$	19.67.

Among liquids the increase is yet more striking: one series will serve to illustrate this.

SUBSTANCE.	MOLECULAR HEATS.
$C\ Cl_4$	31.91.
$Si\ Cl_4$	32.42.
$Si\ Cl_4$	35.25.
$Sn\ Cl_4$	37.15.

I have traced out relations of this kind in over twenty different series of compounds, and find only one or two apparent exceptions. These exceptions are doubtless due to experimental error, since they occur only where there are scanty materials.

Mr. J. E. HILGARD read a memoir communicated by Prof. STEPHEN ALEXANDER, of Princeton,

ON THE ZODIACAL LIGHT.

(See the Appendix to this Bulletin.)

Mr. L. D. GALE read a paper

ON THE GEOLOGY OF THE LIGNITE FORMATION, AND ON A HITHERTO UNDESCRIBED DEPOSIT DISCOVERED IN 1834 IN NEW YORK BAY.

Mr. E. S. HOLDEN read a memoir

ON SIR WILLIAM HERSCHEL'S OBSERVATIONS OF THE SATELLITES OF URANUS.

(*See the Appendix to this Bulletin.*)

Mr. J. E. HILGARD explained the construction of

A NEW APPARATUS FOR THE INVESTIGATION OF PERSONAL ERROR IN ASTRONOMICAL TRANSIT OBSERVATIONS.

72D MEETING. JUNE 20, 1874.

Vice-President W. B. TAYLOR in the Chair.

Mr. WM. FERREL made a communication

ON THE LAW CONNECTING THE VELOCITY AND DIRECTION OF THE WIND WITH THE BAROMETRIC GRADIENT.

(ABSTRACT.)

Let G = the barometric gradient in the direction in which it is the steepest, estimated by the amount of change in the mercurial column in the distance of one hundred miles;

v = the velocity of the wind per hour;

r = the radius of curvature of the isobar or line of equal barometric pressure;

i = the inclination of the direction of the wind to the isobar on the side of lowest pressure;

n = the earth's hourly angular velocity of rotation in terms of the radius;

l = the latitude of the place.

The following equation then expresses very nearly the relation in all cases between the barometric gradient and the velocity of the wind:—

$$G = \frac{(2n \sin l + u)\, v \sec i}{8300000} = \frac{(0.524 \sin l + u)\, v \sec i}{8300000}$$

in which

$$u = \frac{v \cos i}{r}$$

This relation expresses a general law applicable in all latitudes to any individual cyclone, or to the resultant of any number of cyclones combining and interfering with one another. The value of i depends mostly upon friction, and can only be determined by observation. Its value, however, is generally so small, except on certain parts of the land and near the surface, that it may be neglected without much error, since sec i for a small angle does not differ much from unity. The average value obtained by Mr. Ley for inland stations* is nearly 30°, although Professor Loomis (*Silliman's Journal*, July, 1874) obtained an average value of about 45° for the stations of the United States Signal Service.

The mathematical demonstration of this law or relation is too complex to be given here, and must consequently be reserved for future occasions, in which a complete and thorough investigation of this interesting subject will be attempted. For the present it may be regarded as an empirical law, merely to be compared with and tested by observation.

We shall give here only a few comparisons of this law with observation. First with regard to the two great polar hemispherical cyclones having the terrestrial poles for their centres and the equatorial calm belt for their external limits, and depending upon the mean constant difference of temperature between the equatorial and polar regions. In this case the barometric gradients and velocities are the means obtained from a great many observations, in which the effects of the seasons and of all ordinary cyclones are eliminated. The value of u in this case is the mean constant east or west angular velocity of the wind, and r is the distance from the earth's axis. The whole value of u, however, in this case is so small in comparison with $2\ n$ sin l that it may be entirely neglected.

Observation shows that about the parallel of 35° in the northern hemisphere, and a little nearer the equator in the southern hemisphere, the barometric pressure is a maximum, and that consequently G vanishes. Putting $G = 0$, the preceding relation can only be satisfied by $v = 0$, that is, by a calm belt at these latitudes as actually observed. At the equator also we have $G = 0$, but here the law is also satisfied by the vanishing of $(2\ n \sin l + u)$ at the equator, and v becomes in this case indeterminate by the preceding relation.

The mean barometric gradient in the British Islands, as declined from the isobars determined by Mr. Glaisher, is about 0.02 of an inch. With this value of G the relation is satisfied with $v = 6$ miles, supposing the mean direction of the wind to be from the west or nearly so, that is, that the value of i is small. This result is a little less than the value obtained for these islands by

* Journal of the Scottish Meteorological Society for the first quarter of 1873, p. 66.

the late Professor Coffin in his Winds of the Northern Hemisphere. But of course this value, depending upon so small a gradient, cannot be actually determined from the observed gradient by the preceding relation. Again, the value of G deduced from the table of barometric pressures given by Buchan in his Meteorology, for the parallel of 20° north, is about 0.02 of an inch. The wind in the Atlantic Ocean in this latitude being from the N. E., we have the value of i here about 225°. With this value of i and the preceding value of G, the equation gives $v = 15$ miles, which is probably about the velocity of the trade winds on the sea in this latitude.

From the same table of barometric pressures given by Buchan we deduced, for the parallel of 52° south latitude, the value of $G = 0.07$ of an inch. Supposing i here to be small, that is, that the constant wind blows nearly from the west, the relation above gives $v = 21$ miles for the velocity of the wind from the west. All accounts represent the west wind in this latitude as blowing very strong all round the globe, and Mr. Laughton speaks of it as frequently amounting to a half gale.

In an ordinary cyclone the value of u in comparison with $2n \sin l$ cannot in general be neglected, and it is readily seen that near the centre of a cyclone where r is small the value of u may be very large. When the isobars of a cyclone drawn to every tenth of an inch of the barometer are one hundred miles apart, we have $G = 0.1$ of an inch. With this value of G, supposing the value of i to be so small that we can put $\sec i = 1$, we get from the expression of G at the distance of four hundred miles from the centre of the cyclone, and on the parallel of 45°, $v = 29$ miles for the velocity of the wind, and this would be very nearly the velocity at sea where i is so small. But with the value of $i = 45°$ nearly obtained by Professor Loomis, we get $v = 21$ miles nearly. At a distance of one hundred miles only from the centre, all the other circumstances remaining the same as above, we get $v = 22$ miles in the case in which i is small, as at sea, and in the case in which the value of i is 45° we get $v = 18$ miles nearly. Hence we see that the law gives the velocities for the same gradient considerably greater near the centre than at greater distances.

The preceding law cannot be tested by comparing the observed velocity of the wind in any individual case with that deduced by means of the law from the observed gradient obtained from the isobars laid down on the weather maps of the Army Signal Service; for these, being laid down from observations made at stations which are in many cases several hundred miles apart, cannot take in the effects of the numerous local and comparatively small disturbances, and these latter may affect very much the velocity of the wind at any station. The law can only be tested by comparing the average velocity of a great many in-

dividual cases with the average of the corresponding observed gradients, as deduced from the isobars, taking account of the distances of the stations from the centres of the cyclones.

Mr. G. K. GILBERT made a communication

ON THE AGE OF THE TONTO SANDSTONES.

(ABSTRACT.)

The author discussed the age of a group of rocks exposed in the grand cañon of the Colorado, and locally designated as the Tonto group. After mentioning its reference to the Silurian by Dr. Newberry, and to the Carboniferous by Prof. Powell on stratigraphical grounds, he detailed some evidence, paleontological and lithological, which led him to refer it to the Primordial division of the Silurian, and pointed out the interest that would attend the recovery of a fauna from some still lower beds discovered by Prof. Powell under the Kaibab Plateau.

(This communication will appear in full in the Geological portion of the Report of Lieut. Wheeler's Explorations west of the one hundredth meridian.)

Mr. CLEVELAND ABBE read a paper

ON THE POSITION OF THE PLANES OF CERTAIN NEBULÆ.

(ABSTRACT.)

The author stated that he had endeavored to pass from the consideration of the apparent distribution of the nebulæ in space up to the solution of more definite and tangible problems.

He had made a study of the nebulæ described in Sir John Herschel's "General Catalogue" as very much or exceedingly extended, and had computed the position in space of a line perpendicular to the planes of these nebulæ, supposing the latter to be thin disks seen edgewise.

He found that these lines appear to lie in or near a plane inclined about twenty degrees to that of the Milky Way.

(This memoir will be published in full in the American Journal of Science and Arts.)

Mr. E. B. ELLIOTT made some remarks

ON THE CREDIT OF THE UNITED STATES, AS SHOWN BY THE VALUE OF ITS SECURITIES.

APPENDIX.

I.

ON THE ADOPTED VALUE OF THE SUN'S APPARENT DIAMETER.

By E. S. HOLDEN.

(Read 1874, January 17.)

In all, 3639 observations of both horizontal and vertical diameters of the sun have been examined, distributed as follows:—

		No. of Observations. Hor. Diam.	Vert. Diam.	Instruments.
Greenwich,	1862–1870,	832	905	Transit Circle.
Washington,	1862–1865,	491	430	Transit and Mural.
"	1866–1870,	490	491	Transit Circle.
		1813	1826	

These were made by twenty-three different observers. Addendum "A" gives the corrections to both horizontal and vertical diameters as given by all the observations made in one year by each observer. The Greenwich observations are compared with the Nautical Almanac value; the Washington observations with the value given in the American Ephemeris.

From Greenwich and Washington observations of horizontal diameter, the concluded value is 32′ 2″.509
From similar obs. of vertical diameter 32 2 .565
The concluded value is 32 2 .54
giving a correction — 1″.46 to the Amer. Ephem.
" — 1 .08 to the Naut. Almanac.
" + 0 .14 to the Berlin Jahrbuch.

Addendum "A" contains the figures upon which this determination depends. Owing to a difficulty of determining from the printed observations the observer with the transit instrument at Washington in the years 1862–65, all the observations of each of these years have been treated as if made by a single observer.

Each observation of the sun gives an "Apparent error of ephemeris diameter" (horizontal and vertical). Addendum "A" contains the "error of observer" (by changing signs). If we now subtract from the "Apparent error of ephemeris diameter" the "Personal error of the observer" for each day, we have a series of "outstanding errors" which are due to some cause exterior to the instrument itself, and from which the influence of the observer's personality has been eliminated.

When these residuals are tabulated and their means by months taken, as in Addendum "B," it is seen that there is a periodicity in the series. That this periodicity is not wholly or in main

caused by a periodic change in the sun itself, is shown by the fact that these mean monthly "outstanding errors" have on the whole opposite signs at Greenwich and Washington; hence it is inferred that they are in great part due to atmospheric causes. To test this the Washington observations from 1867 to 1870 have been tabulated in Addendum "C," where I have placed the mean "outstanding error" of each year under one of the numbers I to V. These numbers are the observer's estimate of the goodness of the image of the sun at the time of observation, I being a poor image and V a perfect one. It is plain from this table that the goodness of the image has a direct influence upon the value of the diameter deduced, as Wagner first showed in his own case. (*Vierteljahrs. Astr. Gesell.*, 1873, Jan.) I treated Wagner's own observations, which are given in the cited work, in the same manner as I had previously treated the Washington observations, and I found that Wagner's class 4 (on a scale from 1 to 6) gave mean outstanding error of — 0ˢ.055, while the Washington class II–III, which exactly corresponds to it, gave — 0ˢ.063: also, his class 3 gave + 0ˢ.029, while the corresponding Washington class (III–IV) gave + 0ˢ.180.

The agreement of signs shows that the influence of the goodness of image on deduced horizontal diameters is in the same direction at Washington and Poulkova: the discrepancy in amounts is probably due to different habits in assigning the weights.

To show how rough a division of observations on account of state of the image will show an influence in the deduced diameters, I divided the Washington observations into two classes: 1st, those made when the sky was perfectly clear; and 2d, those made when, according to the estimates of the watchmen of the Observatory, the cloudiness was from 5 to 10 (10 = wholly cloudy). The results are given below :—

TABLE OF OUTSTANDING ERRORS IN SUN'S DIAMETER AS AFFECTED BY THE CLOUDINESS OF THE SKY AT NOON.

	Horizontal Diameter.		Vertical Diameter.	
Year.	Cloud = 0.	Cloud = 5 to 10.	Cloud = 0.	Cloud = 5 to 10.
1867	— 0ˢ.033	+ 0ˢ.012	— 0″.45	+ 0.35
1868	— 0 .047	+ 0 .030	— 1 .45	+ 0.45
1869	0 .000	+ 0 .039	+ 0 .75	+ 0.11
1870	— 0 .093	+ 0 .014	— 0 .70	— 0.44

I applied the same test to Greenwich Obs. for 1866, and found:

	Horizontal Diameter.		Vertical Diameter.	
	Cloud = 0.	Cloud = 5–10.	Cloud = 0.	Cloud = 5–10.
1866	— 0ˢ.022	+ 0ˢ.010	+ 0″.55	— 0″.41

which agrees in horizontal diameter with Washington Observations, but is contrary in vertical diameter.

ADDENDUM "A."

Corrections to Sun's Horizontal Diameter. (Greenwich.)

OBSERVER.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	MEAN.
	s	s	s	s	s	s	s	s	s	s
C. …………	—0.05	+0.01	+0.01	+0.01	+0.01	—0.04	—0.04	—0.04	—0.04	—0.019
J. C. ………	—0.18	—0.23	—0.17	—0.19	—0.14	—0.17	—0.21	—0.19	— .20	—0.186
E. …………	—0.13	—0.13	—0.05	—0.09	—0.11	—0.12	—0.08	—0.10	— .09	—0.100
H. C………	..	...	...	...	—0.20	—0.20	—0.20	—0.20	— .20	—0.200
S. …………	—0.10	—0.10	—0.10	—0.10	—0.10	—0.10	—0.10	—0.10	— .10	—0.100
T. C. ………	—0.15	—0.15	...	—0.15	...	...	...	...	...	—0 150
K. …………	...	...	...	+0.02	+0.02	+0.02	+0.02	...	...	+0.020
R. …………	—0.13	—0.08	—0.03	...	...	...	...	...	...	—0.080
G. K………	...	...	...	...	...	...	—0.05	+0.03	+ .07	+0.033
N. …………	—0.13	—0.13	...	...	...	...	...	...	...	—0.130
D…………	—0.06	—0.06	—0.04	—0.06	—0.04	—0.07	—0.06	—0.09	— .09	—0.063
L…………	...	...	...	...	...	...	...	...	— .10	—0.100
P…………	...	...	—0.20	...	—0.19	—0.19	—0.19	...	— .21	{ —0.192 —0.210
W. C. ……	...	...	...	...	...	...	...	...	— .18	—0.180
Sums………	—0.93	—0.87	—0.58	—0.56	—0.75	—0.67	—0.91	—0.69	—1.14	
Means…….	—0.116	—0.109	—0.083	—0.080	—0.094	—0.109	—0.101	—0.099	—0.114	

ADDENDUM "A."—Continued.

Corrections to Sun's Vertical Diameter. (Greenwich.)

OBSERVER.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	MEAN.
	"	"	"	"	"	"	"	"	"	"
C.	—1.2	—1.2	—1.2	—1.2	—1.2	—1.8	—1.8	—1.8	—1.8	—1.5
J. C.	—2.5	—3.2	—3.2	—2.7	—2.5	—2.6	—2.9	—4.7	—3.9	—3.1
E.	+0.2	—0.2	+0.5	+0.9	+0.2	+0.2	+1.0	+0.2	+1.0	+0.4
H. C..........	...	...	...	...	—0.2	—0.9	—1.5	—2.1	—2.8	—1.5
S...............	+0.3	+0.3	+0.3	+0.3	+0.3	+0.3	+0.3	+0.3	+0.3	+0.3
T. C.	—0.4	—0.4	...	—0.4	...	...	...	...	...	—0.4
K..............	...	...	...	—0.5	—0.5	—0.5	—0.5	...	...	—0.5
R...............	—0.2	+0.3	+0.8	...	...	...	...	...	...	+0.3
G. K..........	...	...	...	...	...	...	—1.2	—0.0	+1.0	—0.1
N.	—1.1	—1.1	...	...	...	...	...	...	...	—1.1
D.	—1.6	—2.2	—2.4	—2.1	—2.4	—2.2	—2.3	—2.3	—2.3	—2.2
L.	...	...	...	...	...	...	...	...	+1.0	+1.0
P.	...	...	—1.2	...	—2.0	—2.0	—2.0	...	—0.2	—1.8 & —0.2
W. C..........	...	...	...	...	...	...	...	...	—1.3	—1.3
Sums	—6.5	—7.7	—6.4	—5.7	—8.3	—9.5	—10.9	—10.4	—9.0	
Means	—0.81	—0.96	—0.91	—0.81	—1.04	—1.19	—1.21	—1.49	—0.90	

ADDENDUM "A."—Concluded.

Corrections to Sun's Horizontal Diameter. (Washington.)

Observer.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	Mean.
	s	s	s	s	s	s	s	s	s	s
N.	...	...	...	...	—0.140	—0.141	—0.164	—0.180	...	—0.156
H.	...	...	...	...	—0.118	—0.082	...	...	...	—0.100
Ha.	...	...	...	...	...	...	—0.160	—0.160	—0.140	—0.120
R.	...	...	...	...	+0.008	...	...	...	...	+0.008
T.	...	...	...	...	—0.088	—0.026	—0.088	—0.100	...	—0.076
A.	...	...	...	...	...	—0.140	—0.132	...	...	—0.136
F.	...	...	...	...	...	...	+0.018	—0.040	—0.080	—0.034
S.	...	...	...	...	...	...	...	...	—0.140	—0.140
E.	...	...	...	...	...	...	—0.032	...	—0.080	—0.056
Sums	...	...	...	...	—0.338	—0.389	—0 558	—0.480	—0.440	
Means	—0.126	—0.132	—0.058	—0.014	—0.085	—0.097	—0.092	—0.120	—0.110	

Corrections to Sun's Vertical Diameter. (Washington.)

Observer.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	Mean.
	"	"	"	"	"	"	"	"	"	"
N.	...	...	...	...	—2.76	—3.18	—3.80	—3.00	...	—3.19
H.	...	...	...	...	—2.46	—1.40	...	...	...	—1.93
Ha.	...	...	...	...	...	...	—0.60	—1.40	—2.80	—1.60
R.	...	...	...	...	—0.14	...	...	...	...	—0.14
T.	...	...	...	...	—1.42	—0.56	+0.16	—0.20	...	—0.51
A.	...	...	...	...	...	—0.56	—1.16	...	...	—0.86
F.	...	...	...	...	...	...	—1.36	—3.00	—1.20	—1.85
S.	...	...	...	...	...	...	...	...	—2.20	—2.20
E.	...	...	...	...	...	...	—0.04	...	—1.60	—0.82
Sums	...	...	...	...	—6.78	—5.70	—6 80	—7.60	—7 80	
Means	—0.27	—1.28	—1.88	—1.56	—1.69	—1.42	—1.13	—1.90	—1.95	

ADDENDUM "B."

Mean "Outstanding Errors" in Horizontal and Vertical Diameters. (Greenwich, 1862–1870.)

I. Horizontal Diameter. (*Mean Error for the Nine Years for each Month.*)

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
$+0^s.036$	$+0^s.030$	$-0^s.009$	$-0^s.028$	$-0^s.020$	$+0^s.003$	$-0^s.006$	$-0^s.020$	$-0^s.016$	$-0^s.018$	$+0^s.029$	$+0^s.034$

II. Vertical Diameter. (*Mean Error for the Nine Years for each Month.*)

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
$+0''.44$	$+0''.73$	$-0''.03$	$-0''.00$	$-0''.38$	$-0''.24$	$-0''.42$	$-0''.06$	$+0''.04$	$-0''.18$	$+0''.42$	$+0''.88$

ADDENDUM "C."

Mean "Outstanding Errors" in Horizontal and Vertical Diameter. (Washington, 1867–1870.)

I. Horizontal Diameter. (*Mean Error for the Four Years for each Month.*)

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
$-0^s.024$	$-0^s.004$	$-0^s.059$	$+0^s.001$	$+0^s.034$	$-0^s.015$	$-0^s.002$	$+0^s.046$	$+0^s.016$	$-0^s.022$	$+0^s.029$	$+0^s.023$

II. Vertical Diameter. (*Mean Error for the Four Years for each Month.*)

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
$-0''.18$	$+0''.02$	$-0''.17$	$-0''.55$	$-0''.13$	$+0''.01$	$+0''.38$	$-0''.27$	$-0''.24$	$-0''.65$	$-0''.34$	$+0''.80$

N. B. The mean outstanding errors, including Washington 1866, which have been formed, do not change these means by any important quantity.

ADDENDUM "D."

Table showing the Mean Outstanding Errors as dependent upon the goodness of the Image.

I = very poor; V = perfect. (Washington, 1866–1869.)

WEIGHTS.	I.		II.		III.		IV.		I—II.		II—III.		III—IV.	
YEAR.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.	H. D.	V. D.
	s	″	s	″	s	″	s	″	s	″	s	″	s	″
1866	—0.130	—1.06	—0.024	—0.06	+0.008	+0.39	+0.045	+0.68	...	...	—0.120	—1.80	+0.180	—0.20
1867	+0 015	—0.33	—0.010	—0.44	+0 021	+0.01	+0ˢ.013	—0.11	+0.100	+0.40	—0.105	+0.93	...	...
1868	—0.032	—0.32	—0.026	—0.76	+0.033	+0.60	...	...	—0.040	+1.80	—0.016	—0.32	...	..
1869	—0.100	+0.10	—0.028	+0.24	+0.047	+0.30	...	...	...	...	—0.013	+0.66	...	...

II.

ON THE ALLUVIAL BASIN OF THE MISSISSIPPI RIVER STYLED THE DELTA.

BY PROF. C. G. FORSHEY.

(READ 1874, MARCH 28.)

The Mississippi River has a hydrographic basin embracing 1,244,000 square miles.* The rainfall upon this area, and its abrasions in travel towards the Gulf, have thrown upon the basin below Cape Girardeau, an alluvial bed, extending thence southward to the sea, whose area is computed by Prof. Forshey at 38,706 square miles, about $\frac{1}{32}$ of the entire hydrographic basin.

The river runs through this basin, its present channel skirting the bluffs, or diluvial uplands, on the east, from the head of the Delta, as above, to the south boundary of Tennessee, lat. 35°, a few miles below Memphis; thence to the mouth of the Yazoo River, above and near Vicksburg, lat. 32° 22′, the channel winds midway through the alluvion, and again impinges the bluffs with its east bank; thence it keeps close to the bluffs to Baton Rouge, Louisiana, lat. 30° 28′; and thence to its mouth, at lat. 29°, winds through the alluvial bed 245 miles, projecting more than 100 miles into the Gulf of Mexico.

DELTA BOUNDARY.—The alluvion is bounded on the left or eastern limit by bluffs of some elevation even where the river does not impinge upon them; whereas, the western limit of its basin is very obscure in most of its length, and, though easily recognized by the eye of the geologist, the uplands rise so gently above the alluvial level as to present no definite boundary to the floods that annually cover large areas of the alluvion.

On the west or right boundary the limit may be run from the river at three miles below Cape Girardeau, lat. 37° 20′, southwestwardly through the White Water Creek and the Castor Lakes to the St. Francis River; thence across the bottom of this river to Black River, and with the right of its alluvion down to White River, and with the western limit of its alluvial bottom to the confluence of this and the Arkansas River. Thence the limit runs up the Arkansas to near and below Pine Bluffs, across to the Bartholomew, with its right bank to the Washita, and down west of the alluvion of this river to Harrisonburg, lat. 34° 48′; thence the alluvion basin is bounded by bluffs to the north shore of Lake Ocatahoula and its outlet, the Saline, to Red River;

* In an article published in the Concordia Intelligencer, Jan. 20, 1840, Prof. Forshey computes the area of the basin at 1,300,000 miles The Delta survey of Humphreys and Abbott gives 1,244,000 provisionally adopted till more accurately determined.

thence up the Red River to Cotile, and down Cotile and Bayou Bluff to the Cocodrie, and with its right bank and the Zeche and Vermilion to the Gulf of Mexico, longitude 92° west.

The eastern limit on the Gulf is at longitude 89°, and the width of the basin on the Gulf front is nearly 180 miles, while at Natchez it is 28 miles; at Gaines' Landing, lat. 33° 30′, it is 90 miles, and but little less above that point to near the mouth of Ohio River.

EXCEPTIONS.—Within this area—nearly all below the level of the Mississippi's high water, there are various areas that are not alluvial. Some ridges have considerable length, such as the Bloomfield in Missouri, and the Crowley Ridge in Arkansas, more than 100 miles long and very narrow, lying between the St. Francis and Black rivers; also the Bastrap and Macon Hills in north Louisiana, and the Sicily Island and Avoyilles on Ouachita and Red Rivers. All these are subtracted in computing the area of the alluvial basin, still leaving a tract of land with an average of 12 feet below the highest water mark of the Mississippi River at points opposite, and measuring 38,706 square miles.

This is the extent in greatest floods of the *Fresh water* DELTA SEA.

APPARENT GEOLOGICAL HISTORY.—The testimony left on the features of the land shows the drift beds in the form of bluffs on both sides the basin, and of indefinite lateral extent. The drainage of the great hydrographic basin has unquestionably eroded the bed now occupied by the alluvion; and whether in concentrated form as now, or in several or many parallel streams, the Mississippi has cut out for itself the delta basin, and thrown down upon its lap the varied detritus brought from the abrasions of its many tributaries.

The diluvial beds thus worn away lie upon a tertiary, at various points exposed on either side of the basin. The depth to which the bed has been cut (the thickness of the alluvion) is ascertained by only a very few sections, but sufficient to warrant the conclusion for all its northern half, say above lat. 32° 30′, that this depth is rarely greater than the deepest portions of the present shifting channel, about 100 feet.

Ample testimony exists, and it is everywhere observable, that the river changes its bed with ease and rapidity, showing that the material removed to give the channel-way is of recent alluvial deposit, and that at least so far as these changes have occurred, the depth of the alluvion has for a minimum 100 feet or more.

At points further down, we know not at what limit, a bay from the Gulf has been gradually filled with alluvial material, the marine waters being driven back, and the deposits cast on the bottom of the sea, until at the present age, the alluvion has greatly invaded the basin proper of the Gulf of Mexico.

The protrusion along the margin of the Gulf is manifestly eighty miles or more, if we assume the line of the convex shore of the Gulf to be continued from west of Vermilion Bay to the shore of the Mississippi Sound at Bay St. Louis. This line passes through the city of New Orleans.

In the progress of the stupendous work of filling the whole Gulf area* of 700,000 square miles, and of average depth of 3000 feet or more, by the Mississippi and its auxiliary streams, the delta we are now considering has projected beneath the present Gulf surface an enormous plateau with less than ten fathoms water soundings, which extends from five miles outside the present passes of the Mississippi, westward, and has a breadth in front of the Caillon and Atchafalaya of more than sixty miles, and only approaches the shore west of the Vermilion Bay, the western limit of the delta proper.

Judging from the bottom samples brought up by the soundings, the alluvial materials characteristic of the Mississippi's contributions lie upon and form the bottom of the Gulf for some seventy miles still further advanced, in a crescent southward and eastward of this ten-fathom curve, even to where the soundings reach beyond 500 fathoms.

This, then, is the provisional southward or Gulf limit of the great alluvion.

TOPOGRAPHY OF THE BASIN.—The river's banks are everywhere the highest ground within the basin, including in this description of the banks all the shores of the Old River lakes which are so numerous along its tortuous course.

And as the river has elevated or built up the alluvion by dropping upon its lap the sedimentary and transported contributions eroded from the great hydrographic basin, it will be obvious to the student that the highest portions of these banks are found to be nearly on the level (a little below) the greatest high-water marks, as we shall see below from the chapter on the physics of the river.

A line of levels transverse to the current, which runs mainly southward, shows everywhere an undulating surface to the ground; for "the delta is everywhere threaded by interlocking bayous and navigable channels, placing every cultivable acre of land immediately upon or very near to steamboat navigation. In this

* According to Humphreys and Abbot, the contributions of the river, as held in suspension, would raise a square mile 241 feet high. Add five times this amount for material pushed forward on the river bottom, and we have $241 \times 6 = 1446'$ as the height of the one mile block. With a gulf depth of 3000 feet our contributions would fill half a square mile per year, and would fill the Gulf area of 700,000 square miles in 1,400,000 years. But as the other streams discharging into the Gulf bear probably one-fourth the amount of sediment furnished by the Mississippi, this period would be reduced to nearly a million years.

particular it has no parallel known to civilized man." These bayous and rivers have their channels mainly parallel to the river's course, and they serve as drains to the rain-waters falling upon the delta; and before levee history, and partially now, they serve to bear off the waters that overflow the lower portions of the banks of the Mississippi itself.

The depths of the greatest undulations below the river's high-water level are about thirty-seven feet, though such depressions are very rare. Ten, twelve, and fifteen feet are much more common; but all the depressions are greater at the greater distances from the present channel.* The average depth reduced from all the sections leveled, is about 12 feet. The delta sea, if raised to the level of the river, at like latitudes, would have a mean depth of 12 feet. In all great floods, then, the entire delta is menaced by the river's flowing reservoir of 12 feet average depth of water. Thus much does the great river lack of having filled and replaced with deposits what has been carried away by its erosions.

PECULIARITIES OF THE PHYSICS OF THE RIVER.—The mode by which the river Mississippi produces the formation of the alluvial delta, is not in general terms different from that of any river or rivulet, whether great or small.

In some particulars the mode is peculiar; and as these peculiarities have been determined by the most elaborate investigation yet given to any great river, the writer upon these phenomena can speak with confidence, and may once for all give credit to the great work of Humphreys and Abbott on the "Physics and Hydraulics of the Mississippi River."

The greatest of these peculiarities is found in the perpetual turbidness of the waters. Three rivers from the west, the Missouri, the Arkansas, and the Red River, contribute turbid waters in both flood and low-water seasons. The Missouri, however, yields the greater portion of the transported matter, and gives the muddy hue to the waters; the other two rivers yield ferruginous matter, tinged with red, that modifies the color of the river below them, especially in flood seasons of these rivers.

The Mississippi bears in suspension an alluminous clayey matter, of extremely fine comminution, that amounts in quantity to about one part by weight in 1800, and by measurement one volume in nearly 3000, as shown by many thousands of careful tests, carried through two entire years, and derived from all portions of the river's flowing volume.†

* The river bank of the Washita, at 75 miles west from Vicksburg, is 12 feet below the Mississippi's bank; and on this line of levels, at only 12 miles east of the Washita, the land is 38 feet below the Mississippi level; the greatest depression found in the 15 lines of level run across the delta.

† The writer hereof performed these measurements, as well as others, as the Hydrometric Engineer's Assistant of the Delta Survey.

In addition to the matter transported in suspension by the river's current, there is borne forward by the mechanical force of the waters a vast volume of material of coarser and heavier kind, which is rolled and pushed along the bottom of the channel, believed by the writer to exceed in volume the suspended matter by five (possibly twenty) fold. Its quantity is immeasurable, but proved to be vast.

As moving water in any river has many oblique currents, due to irregularity of channel surfaces, much of the transported mineral matter is kept in suspension, in spite of its specific gravity. In consequence, the forward-moving matter may be, and often is, lifted and dropped again and again, thousands of times, in its progress. Whenever any volume of water drifts into an eddy, or finds a slower movement, portions of its sediment are dropped, only to be lifted again when impinged on by an oblique and upward current. Along a bottom of ascending plane, even heavy materials are rolled or pushed upward to greater elevations, even to the river's surface, and thus frequently are thrown out upon the highest banks, where obstructed currents can no longer drift them forward; and they thus form permanent additions to the land.

It is thus that the immediate front banks of the river are highest. The escaping waters, retarded by obstructions, leave their heaviest load of sand near the banks, and bear forward the lighter materials into the lower grounds, back from the front banks. And thus is realized and explained the familiar fact that the front surface-lands are of heaviest and often coarse sandy material, while the remoter surfaces are of more tenacious and clayey composition.*

The river and its auxiliary bayous have thus occupied, at various periods, nearly all the areas of the delta, building up, carving away, and replacing the beds of alluvion, varying in character from coarse sand to the finest blue clay, all over the basin.

It is obvious, in this process of changing its channel and renewing its walls, that the river must have many lower and higher portions in its banks, and that in considerable floods the water must be pouring out at many places, and finding its way down the delta through the laterals, and often through the forests, that cover the lower, but mainly emerged, portions of the basin. And hence the necessity, when man claimed the occupancy of the delta, of levees.

Levees. — When the French discoverers pitched their pal-

* It must not be inferred, hence, that these facts appear at all depths in the alluvial basin; for in the shifting of channels, sand-banks are often covered by clayey material, and the converse. The river's caving banks illustrate this in multiplied instances. No rule applies to sections of the alluvion; they differ at every hundred or thousand feet.

metto tents where the city of New Orleans now stands, they found themselves annually liable to floods that they must guard against; and in 1717 the governor ordered his engineer, De la Tour, to throw up a dike round the fort and the proposed city. They commenced planting on the adjacent fertile grounds, and found that crops were impossible without protection; and they then extended the earthen embankments along the river in front and made guard levees running back, to guide the flood waters past them. The new arrivals of colonists planted the land still above and below them, and extended the protection still further. And still they found themselves flanked by the floods, and again new settlers carried the works further and further upwards.

These efforts resulted in laws regulating levees, as communities grew up along the river, and upon the interior streams partly reclaimed by front levees and partly by similar works on the bayous. After the purchase of the country from France, the American population rapidly increased, and extended the settlements upward and into the interior, invited by the extraordinary productiveness of the lands and the mildness of the climate; and laws regulating the levee building and enjoining upon every proprietor to build and maintain his own levee, were enacted and enforced with much rigor.

By the year 1838, when the writer's labors in connection with levees commenced, the protection was pretty complete from sixty miles below New Orleans up to Red River, and a large portion of the river front for one hundred miles above Red River was being protected. And still the flood waters would flank the plantations, as the levees were carried upward and upward. Several large outlets had been closed. Bullitt's Bayou, and L'Argent in Concordia were leveed; and such were the happy effects of these works on the interior country, that in 1840 the writer published the bold proposition to *levee and master the whole alluvial basin.* This was contrary to nearly universal opinion as to practicability, but soon found adherents. The demand for land-dominion fast moulded opinion.

The universal impression, without much reason, that the river must fill up its bed, and cause a constant raising of the levees, began to give way, and we soon (by 1847) ran our levees to the very northern limits of Louisiana.

The theorists, however, nearly all maintained that the proposition of the gradual elevation of the levees was heretical and unsafe. In opposition to this, experience, now a century old or more, was appealed to, which showed points of land near and above New Orleans that were as high (or nearly) as the highest water marks of recent times.

The contest ran through a decade of years, and the triumph of the new doctrine was finally due more to the desire to possess the lands, than to any scientific conviction. In 1851, the great Delta

survey began, which ended in sustaining the grounds then assumed, and announcing the practicability of the total reclamation of the alluvion by levees.

In illustration of the sentiments of those times, I quote from an essay written by myself in 1849, entitled the "Physics of the Mississippi River," deduced from twelve years' observations and studies.

"*State of Levees and their Servitudes.*—24. (*a*) The levees of Louisiana may be regarded as in full operation for fifty years, for a distance of one hundred miles from Bayou Lafourche down below the city. These levees have an average height no greater than those now being erected in the upper portion of the State; and the highest water-marks known, whether within the levee districts or not, are no higher than many points of the land; and some of the best river plantations present long reaches without levees.

"(*b*) The river, therefore, has not raised its bed, nor reached point of elevation, in recent years, greater than its level when it deposited its high grounds.

"(*c*) To maintain levees in future, therefore, we shall have to raise them no higher than in the past.

"25. (*a*) The location of levees below Baton Rouge was chiefly made before those further above, and consequently were placed too near the bank to admit of the new abrasions, arising from cut-offs, from extended levees, and from the never-ceasing steamboat waves.

"(*b*) For this reason they are now being destroyed by caving banks and by lashing waves of steamers and winds.

"(*c*) A period has arrived when these new elements have cut away the small battures; and the high waters, which the geology of this alluvion shows to have been frequent, geologically speaking, in past ages, are recurring, and our levees are wholly unequal to the task of restraining the waters." * * * *

At the beginning of the late war of secession, the levees were extended nearly to the head of the Delta, leaving only intervals near and above the inlet rivers, the Red, Arkansas, and St. Francis, amounting in all to less than forty miles. And while many of these levees were very frail and liable to frequent crevasses, many others, and especially those which closed the great outlets, were substantial dikes.

During the war many of the great levees were cut as a military necessity, and many others broke of themselves, and from neglect the cuts widened, until most of the country was remitted to desolation.

The State of Louisiana undertook to restore her levees, and has expended some twelve millions of dollars since the war in the attempt. But the States of Arkansas and Mississippi have done very little in the work of restoration.

The floods of this spring (1874) have been so extraordinary as to break the levees of Mississippi and Louisiana, and to devastate these States with that of Arkansas by a most disastrous inundation, involving loss of life, property, stock, homes and improvements in one vast ruin, followed by famine and threatening pestilence.

While the flood height in most of the alluvial basin is not greater than in some previous floods, for example in 1815, 1828, 1850, 1862, and 1871, the great extent of population and the bad condition of the levees have given an intensity and breadth to the suffering and devastation, incomparably greater than has ever been experienced on the continent.

The people, in their utter impotence and poverty, turn with prayers of desperation to the National Legislature for bread to save them from famine, and for the strong national arm and purse to rebuild and take charge of their levees, or the people must perish, and the Delta be remitted to the dominion of the annual floods.

A quotation from an essay read by the writer before the American Association at Dubuque, in August, 1872, will form a proper closing for this contribution :—

"The fertility of the soils of the Delta both by analysis and experiment, is of the highest quality ; in fact, it is almost inexhaustible. Accordingly, it produces, in its southern two degrees, the great staples of rice and sugar in abundance and perfection unknown in any other portion of North America. In fact, sugar is cultivated only in the Delta, and south of latitude 31° 30′. In nearly all portions of the Delta, but more thoroughly in the five degrees north from 31° (north of Red River), cotton grows in the Delta lands in double the quantities of the best uplands ; and corn, and sweet and Irish potatoes, in every portion of the Delta, grow with facility and abundance, and with a minimum of cultivation. In the northern border the cereals grow and mature to the satisfaction of the agriculturist. The fruits of the tropical and temperate zones—oranges, figs, grapes, apples, and peaches—are duly distributed and easily grow, each in its proper locality, over the Delta ; while pecans, the most valuable of all nuts, grow in wild profusion over the entire alluvial basin.

"The remarks as to productiveness are applicable to every acre not submerged, and not merely to chosen spots, as upon the uplands adjacent on either side.

"We may compute then that 22,920,320 acres of actually productive land are upon this alluvial basin. In this respect it is probably the largest body of like fertility known to geography.

"The forests of the Delta are remarkable for the largeness of the trees, and the exuberance of foliage and vines.

"The oaks and the cypress are the leading timber trees, though many others are used. The live oaks in the southern

portion are large and very abundant, indicating mainly a soil not often inundated. But the cypress trees of vast height and magnitude, and of unlimited demand, grow best in the lowest swamps, and do greatly redeem and render equally valuable (as cultivated land) the most impracticable portions of the whole valley. Fifty thousand feet of lumber, clear stuff, from an acre of cypress swamp, is no unusual product.

"Freedom from the extremes of heat and cold forms a great feature of this Delta; and distinguishes it greatly above the alluvions of the Nile, the Ganges, the Amazon, and the Orinoco.

"The annual mean temperature at New Orleans, Baton Rouge, Natchez, Vicksburg, Helena, Memphis, and Cairo, shows a regular gradation from 69° to 50°.

"So inviting is the temperature of this Delta, during the largest portion of the year, from the northern limit of the cotton region, southward; and so promptly, uniformly, and abundantly does the soil respond to the labors of the husbandman, that its hundreds of winding streams were lined with settlers before the war, even anterior to any certain protection, by levees, from frequent inundation. It was common to say that a loss of two crops in ten, by overflow, could be better borne than the half crops produced upon the uplands."

III.

ON THE ZODIACAL LIGHT.

By PROFESSOR STEPHEN ALEXANDER.

(Read 1874, June 6.)

As to the region of the zodiacal light M. Laplace, speaking of the atmosphere of the sun, says: "The atmosphere at the equator cannot extend beyond the point where the centrifugal force exactly balances gravitation; for it is manifest that beyond that limit the fluid must itself be dissipated. As respects the sun, that limit is at a distance from his centre of the radius of the orbit of a planet which would complete its revolution in a time equal to that of the rotation of the sun. The atmosphere of the sun, therefore, does not extend even to the orbit of Mercury; and consequently it does not produce the zodiacal light, which seems to extend even beyond the earth's orbit. Moreover, this atmosphere, whose polar axis must be at least two-thirds of that of the equator, is very far from having the lenticular form which observations give to the zodiacal light."*

Next, as to the origin and the constitution of the material which gives us the zodiacal light, Laplace says: "If among the zones abandoned by the atmosphere of the sun, there should be molecules too volatile either themselves to combine, or to unite with the planets, they ought while continuing to circulate about the sun, to present all the phenomena of the zodiacal light, without opposing a sensible resistance to the diverse bodies of the planetary system, either because of the extreme rarity of those volatile molecules, or because their motion is very nearly the same with that of the planets which they encounter."†

It will be observed that the first of the two quotations here made, estimates as probable that the material from which the zodiacal light proceeds, itself extends beyond the earth's orbit. This is in fact intimated by the existence of what in German accounts of observations has been designated as the "*gegenschein*," which is seen in the part of the heavens opposite to the sun; the existence of which phenomenon is established by numerous observations, such especially as are detailed in various numbers of the "Astronomische Nachrichten."

Both eastern and western appearances are reported as occurring simultaneously, by the late Rev. George Jones, A.M., Chaplain in the U. S. Navy; these phenomena are fully reported in vol. III of the Report of the U. S. Japan Expedition, and the extension of the light to both sides of the heavens is confirmed by

* Système du Monde, Book IV, chap. X. † Ibid., Note VII.

the observations of Col., now Prof. C. G. Forshey, made while he was stationed in an elevated and dry region of Texas, where, as stated by Prof. Forshey to the author of this paper, the phenomenon was a common occurrence; the appearance of the zodiacal light in lower Louisiana is very different.

All this makes it more difficult to admit that the material in question can be maintained in position with the sun for its centre of reference; the conservative influence of the great planets being not supposable within the extended limits of the solar system; though the satellites of Saturn are efficient in that way, maintaining the position of the rings under the more stringent conditions of a closer arrangement.

Added to this is the consideration of the enormous extent which would seem to be required on both sides of the ecliptic, to account *for the great breadth of the base* of the zodiacal illumination, even after the disappearance of twilight in the evening, or before daylight in the morning; all which seems to be true of *the more dense*, and, if surrounding the sun, also the more distant portion of the material in question, which ought, unless uncommonly extensive, to be seen under a *smaller angle* than the other portions of the same; a difficulty to which the hypothesis recently advanced by Mr. Richard Proctor, F.R.A.S., viz., that the zodiacal light is due to a closely arranged group of meteors, would seem to be especially liable; and all the more so if, "assuming" (as he himself says "we are bound to do") a considerable degree of flatness in the actual figure of the zodiacal disk, and more especially "of its more distant positions."* And just *that* difficulty still remains; if we even so far admit Prof. Arthur W. Wright's conclusion from his experiments on the *polarization* of the Zodiacal Light, viz.: "that the light is reflected from matter in a solid state."...†

Other objections to the hypothesis which would make the material to which we owe the zodiacal light to be an appendage of a lenticular or other form referable to the sun as its centre, are very exhaustively considered by Rev. Mr. Jones in the volume already referred to.

The hypothesis that the zodiacal light is due to reflection from the earth's atmosphere, is also discussed and rejected by him. Upon this, however, it will not be necessary here to comment; as it, most probably, is no longer insisted upon by any one.

It remains then to consider, *with what modifications* we may admit Mr. Jones's hypothesis: That the nebulous material which gives the zodiacal light, is a terrestrial appendage; and also what is the conservative force which may insure its preservation of

* In a long and carefully considered "Note on the Zodiacal Light" in the Monthly Notices of the Royal Astronomical Society, vol. XXXI, No. 1 (Nov. 11, 1870).

† American Journal of Science, &c., Third Series, Vol. VII, No. 41.

form, and its maintenance in its revolution around the earth, even in close proximity to the moon.

Antecedent to all that, however, will be found to be the question of density, and of mode of illumination, as well as, in its proper connection, the question of parallax.

The density of the material in question seems, indeed, to be that intimated in the description of M. Laplace already quoted, viz.: that which pertains to the state of molecules too volatile either to combine themselves, or to unite with the planets. And this is confirmed by the result of the spectrum-analysis; which has led to no other reliable conclusion than that of the *extreme rarity* of this same material.* The same rarity is withal indicated by the transparency of the material. Of this Rev. George Jones says, under date of Dec. 30, 1854 (in lat. 10° 46′ N., long. 89° 31′ W. of Greenwich), "I also this morning gave attention to the stars as seen through the zodiacal light, and found even at 4^h 30^m, when the effulgent light below the zigzag" (on the chart) "is very strong, that, with the naked eye, I could readily make out stars of the sixth magnitude within the effulgent light, ... also a line of four stars below 19 Libra . . . . the two northernmost of these last are of the seventh magnitude, yet I think the naked eye detected them within this effulgent light; but the last are near its upper edge. All this shows the great transparency of the substance giving the zodiacal light."†

The consideration of these phenomena inclines to the conclusion that the zodiacal light proceeds from particles which are *molecular* (at least in size), and not from discrete, though very small solids. It then must also, largely, be *transmitted* light; and so the illuminated material appear *brighter* in conformity with the special *direction in which the light is* transmitted. Chaplain Jones illustrates this in part—though he refers the light almost entirely to reflection—when he says: "It seems to be quite conclusive, on an inspection of these charts, that *we never at any one time see the whole actual extent of the zodiacal light.*" This can perhaps be elucidated by noticing a common event—a cloud silvered at one edge by the rays of the declining sun. The sun may be shining on the bordering quite around that cloud; and if so, it is sending off, from every part of the border, an equally brilliant, silvery light. But our eye is in a position to catch this reflection from "only one portion of it, and the rest is dull to our vision. If we could, with great rapidity, change our positions, other portions of the silvered edge would show themselves, according to our changes of place. So, also, when a rainbow is presented to our eye; the myriads of drops of falling water, in the whole rain-

* Such was, in effect, the statement of Prof. Charles A. Young (as the result of his experience and that of others), in a personal communication with the author of this paper.

† Vol. III of "Report of Japan Expedition," No. 371, at p. 542.

shower, are sending off from each drop reflections of light in all directions, and the universal atmosphere about us is full of these brilliant, variously-colored rays; but only that portion which to us forms the rainbow arch, can reach our eye, and all the rest is lost to our sight."

"So it is also with the zodiacal light; and the proof that we never see the whole extent at once, is manifest in the following facts:—

"1. That when I was in a position north of the ecliptic, the main body of the zodiacal light was on the *northern* side of that line.

"2. When I was *south* of the ecliptic, the main body of the zodiacal light was on its *southern* side.

"3. When my position was *near* or *on* the ecliptic, the light was equally divided by the ecliptic, or nearly so.

"4. When, by the earth's rotation on its axis, I was, during the night, carried rapidly to or from the ecliptic, the change of the apex, and of the direction of the boundary lines, was equally great, and corresponded to my change of place.

"5. That as the ecliptic changed its position, as respects the horizon, the entire shape of the zodiacal light became changed, which would result from new positions of the nebulous matter coming into position to give visible reflection; while portions lately visible were no longer giving us such a reflection.

"The first four of these results were not always uniform; but the exceptions were few, and were probably occasioned by the nebulous rings not being exactly in the plane of the ecliptic."—(Introduction to Mr. Jones's Report, pp. 16 and 17.)

Mr. Jones in his § x adds to this: "From the deductions made in § I, it is apparent that we cannot expect to get a parallax of this ring; and that we can hope for only approximations to its width."

The partial illumination on the side *toward* the observer was indeed a *negative* to parallax in excess; somewhat as aberration is in its effect on the annual parallax of the stars, though not in the same way.

Mr. Jones might have added, that the circumstances which he mentions would also be in the way of ascertaining the *node* of the nebulous band, and must render questionable any supposed determination of that.

The light, as we have said, being from particles which are molecular, and the whole material from which the light comes to us remarkably transparent, the light itself cannot be regarded as being a mere reflection;* *but the result of the more complicated*

* Mr. Proctor also seems inclined to admit the possibility of a more intense illumination in special directions; though not decided as to its cause, when he says: "If some solar action, for example, powerful luminosity in certain definite directions—as, for instance, near the plane of the

phenomena supposable in the case of transmission. The illumination will therefore be the most intense in the general direction of the transmitted beams; *i. e.*, in this case, in the plane of the ecliptic, or else parallel to that plane; the appearance being closely analogous to that which we see in the atmosphere, when the sun illuminates the partially transparent vapors through the rifts in the clouds, and thus produces the appearance familiarly described as "the sun's drawing water."

The light being transmitted, other phenomena would also be in place, among which are *absorption*—possibly *interference*, and also fluorescence; new waves of light being originated in this case, as well as, perhaps, in that of the comets, the spectrum-analysis of whose light seems to show, among other phenomena, characteristics of self-luminous material.

To this it may now be added that the nebulous ring of chaplain Jones may well be regarded as having not indeed the lenticular form attributed by older hypotheses, and which he does not claim, nor a ring like that of Saturn, nor yet a *ring* of greater thickness, though partially luminous indeed in appearance, as Mr. Jones would have it, *but what may rather be termed a girdle, of no great thickness it may be, but yet of very considerable width;* such as will provide for the breadth of the zodiacal light, and the extended elliptical spot opposite to the sun, which constitutes "*gegenschein*," and which latter would seem to be almost wholly due to reflection. Such a girdle, moreover, could not always—perhaps hardly ever—have all its breadth enveloped in the earth's shadow. There may also be some reason to suppose that the curvature of the girdle, on one side at least, is such as would be due to a portion of a spheroidal shell such as has heretofore been described in connection with other things connected with the exposition of certain harmonies of the Solar System.

But the question at once becomes a pertinent one, How can such a girdle escape destruction by the continued perturbation of the moon, acting in close proximity? The answer to this question may be found, *if the girdle be so situated that its time of revolution around the earth shall be equal to the periodic time of the moon.*

What is requisite for the fulfilment of that condition will first be considered, and then the phenomena that seem to be accordant with its actual fulfilment.

The middle line of the girdle will form an oval, or rather consist of two half ovals somewhat different in curvature. The half oval nearest the moon may pass between the moon and the earth, as in Fig. 1, or else outside of the moon, as in Fig. 2; in both

sun's equator—in some such way as light is caused to appear in radial lines through and beyond the heads of comets, our power of theorizing from such conclusions as have been dealt with in this paper, would be limited."

of which E represents the earth and M the moon. Now it first becomes convenient to ascertain the periodic time of a particle, or

Fig. 1.

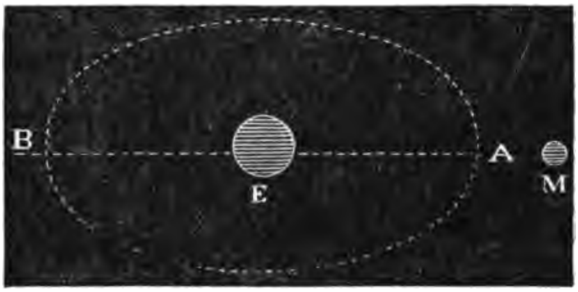
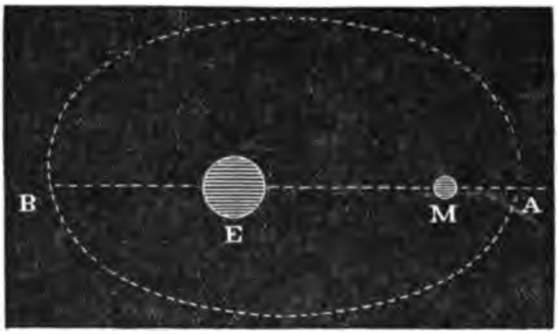


Fig. 2.



of an inappreciable mass, revolving at the mean distance of the moon; and this may be ascertained by means of the formula for the periodic time which we designate as (T); M and m being the masses in question; and we shall have

$$(T) = \frac{2\pi r^{\frac{3}{2}}}{\sqrt{M+m}} \quad *$$

Then when m is insensible,

$$(T') = \frac{2\pi r^{\frac{3}{2}}}{\sqrt{M}};$$

and, when r is the same for both,

$$\frac{(T')}{(T)} = \frac{\sqrt{M+m}}{\sqrt{M}}; \text{ whence}$$

* Encyclopædia Metropolitana, Physical Astronomy, Sect. v.

$$(T') = \frac{\sqrt{M+m}}{\sqrt{M}}(T).$$

Having determined the periodic times of a particle, or of an insensible mass, by the aid of this, we may next ascertain the periodic time at an assumed distance E A, situated as in Fig. 1, by the application of Kepler's third law: let this be represented by (t).

Now let the attractive forces of the moon and the earth, acting at A, be separately computed in accordance with the law of gravitation $\left(\frac{M}{d^2}\right)$, and then, taking the difference of the two forces, let the same be expressed in terms of earth's force F, and let it be denoted by $\frac{p}{q}$ F, and (t') represent the periodic time due to $\frac{p}{q}$ F at the same distance E A. We shall have

$$(t')^2 = \frac{F}{\frac{p}{q}F}(t')^2$$

If $(t')^2$ thus ascertained is found to agree with the moon's periodic time, the point A is well determined. If there is any difference, that may be made to disappear by the application of the method of trial and error. When A is situated beyond the moon, the sum of the attractive forces must enter instead of their difference; and so also in the determination of the point B.

The positions of the points A and B, on the hypothesis that the girdle on the one side is between the earth and the moon, may be learned from the following table:—

	In Perigee	Mean Dist.	In Apogee
Moon's distance	56.964	60.273	63.583½
Int. dist. E A	48.309	51.116	53.922½
Ext. dist. E B	56.790	60.090	63.389

On the hypothesis that the girdle encompasses the moon, as in Fig. 2:—

	In Perigee	Mean Dist.	In Apogee
Dist. Ext.	66.426	70.285	74.144½

The distances are all expressed in terms of the radius of the earth's equator.

The position of the points in question ultimately depends upon the ratio of the masses; so that the E A and E B of the girdle in apogee, perigee, and mean distance, respectively, preserve their ratios to the moon's own distances, and hence A and B move in ellipses similar to the moon's own orbit; the girdle at those places expanding and contracting. The self-adjusting material

of the girdle will be brought everywhere into coincidence with this; or would be, if the approximate coincidence originally existed. Then the ratios of E A, E B, and even those of the moon's distance from the centre of gravity to them being all constant, the angular velocity (inversely as the square of the radius vector) will also be determined for all the same (and all the radii vectores answering to the same ratio), each series on its own scale; and this involves the very principle of the conservation of areas.

CONFIRMATORY PHENOMENA.

Zodiacal Light:

(1.) Lengthening into a narrow beam when the light is transmitted through the sides of the girdle, as at New Moon, or at the Full.

(2.) Appearing bright, but short and broad at the quadratures.

(3.) Exhibiting a bright spot or patch of light opposite to the moon soon after the full where the longer axis of the oval terminates on that side; which bright spot rises from day to day, as the moon moves on toward her last quarter. It has advanced toward the top of the zodiacal illumination before the last quarter comes.

Observations of these phenomena have as yet been few; but Chaplain Jones's charts seem also to confirm them.

Postscript.—The several parts of the girdle, as they revolve with the moon, all describe ellipses similar to the moon's own orbit; but the outline form of the girdle itself (*i. e.* of its middle line), does not differ much from a circle, except in the region near the moon.

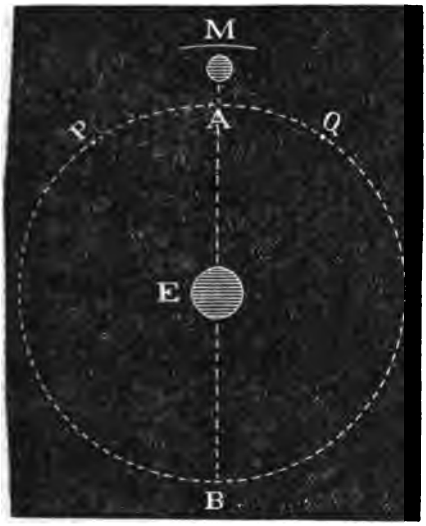
Quoting the preceding table of dimensions, we have :—

	In Perigee	Mean Dist.	In Apogee
Moon's distance	56.964	60.273	63.583½
E A, Int. dist. of Girdle	48.309	51.116	53.922½
E B, Ext. dist. of Girdle	56.790	60.090	63.389

At P and Q, where the moon's attraction is in the direction of a tangent, the accumulation of material may be different from that at places further around toward B, but with the moon's time of revolution enforced, the distance E P (earth's central attraction fully operative) must be just a trifle less than the moon's distance.

Consulting the table, we find *that* is also true of E B. The feeble action of the moon will insist upon a small deviation outward at the sides. But from P, through B around to Q, the existing distance of the moon from the earth's centre will be nearly preserved. But then even *that* requires a swelling out of the girdle

as the moon approaches the apogee, and a shrinking as she goes



from there to the perigee; the different portions A, B, &c., each fulfilling all the conditions of rotation in an ellipse similar (perturbation apart) to the moon's orbit.

The permanent distortion of the girdle by the moon's action is analogous to, and yet different from, an enormous *tidal* action; but with a tide in which the variation of the attractive force in the inverse ratio of the square of the distance, and the variation at different distances of the centrifugal force of revolution have a very great influence. So A, the place of what ought to be the flood tide, is the point *nearest* the earth; the stringent conditions of *slow* time of revolution and contest of forces bringing about that negative tide-like result.

The girdle form is not much troubled with the objection that the earth's shadow ought to cut it in two at the direction of the meridian at midnight.

It will be perceived that the position of the point A is *within* the limit assigned by Laplace to the region where the moon, in ancient times, may have lost her atmosphere. The conditions of revolution, &c. here give a centrifugal aid to the material, which has to go too slow for its mere place, so that the *nearest* point is for mean distance (60.273 — 51.116), *i. e.*, 9.157, or more than one-seventh of the moon's distance within.

M. Laplace has evidently made use of the ratio which would make M A and A E as the square roots of the masses.

It would be very curious certainly, if Saturn's rings, *dusky* and bright, are preserved in place, because that planet has *so many* satellites; and our *nebulous* girdle be kept in place just because we have but *one satellite:* but even that is so much more than belongs to our immediate neighbors.

The earth is, indeed, the Saturn of the *minor system* interior to Jupiter, in which system the asteroids and Mars correspond to Uranus and Neptune of the *major system;* Earth is the Saturn (with satellite, girdle and all); Venus is the Jupiter, and Mercury (including material for more than one planet there) is the representative of all the small planets on the greater planetary seal.

Resemblances and Differences of Saturn and the Earth (the *minor* Saturn).

1. Abnormal Density; Earth's too great for the region, Saturn's too small. The former took its large density from extra-equatorial parts of the sun's atmosphere; latter from material abstracted from Uranus.

2. Great oblateness in the nebulous state of former time, shown by the great distance of satellites in both cases.

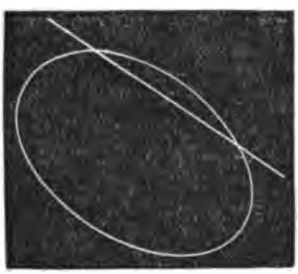
3. Each has more satellites than any other planet in the same region.

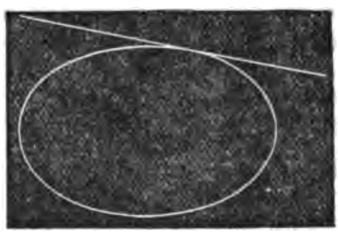
4. Saturn's satellites were formed before his rings, and so kept the latter in place. The earth's moon and the girdle agree nearly in their principal planes and orbit with the plane of the ecliptic; and were, therefore, probably established *before* the earth's axis got its inclined position.

In completing my account of the zodiacal light, I will transcribe here further details of the agreement of my hypothesis with observed phenomena.

CASE 1. The zodiacal light appears narrow and tapering just about the time of the new moon, *as though the sun's light were indeed transmitted at that time through the least curved, and probably somewhat rarer, sides of the oval-shaped girdle, and that for a great part of the length of the oval.*

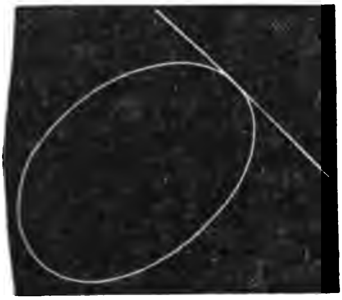
CASE 2. After the new moon, and when the moon is approaching her first quarter; when the moon has set, and the twilight has disappeared, the zodiacal light does not extend so high as in the preceding case, its termination is broader, and not so sharply curved, and the intensity of the light, withal, is not especially conspicuous; *as though the sun's light, in all its transmission, passed through the rather less dense portion of the girdle, and passed out of it in a direction more across the same* (as in the Fig.), and not so nearly at a tangent to it, as under the circumstances indicated in the preceding case.



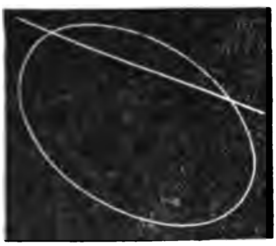


CASE 3. After the Full Moon, and when the moon is approaching her Last Quarter; then before the rising of the moon, and after the end of twilight, a luminous spot of a considerable size,

and in appearance like the brighter portion of an aurora borealis, occupies a place in the zodiacal light which is quite accurately opposite to the moon's place; and, night after night, as the moon advances, this luminous spot rises among the stars, so as still to keep opposite to the moon; *as though the somewhat more dense portion of the further end of the oval (as respects the moon) were thus more conspicuous than the other portions then in view: and then, the upper extremity is broad, and not so sharply pointed as is true in Case* 1, *as though for the reason assigned in Case* 2.

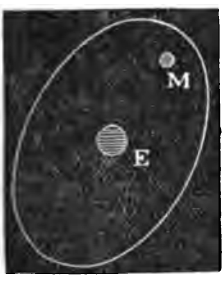


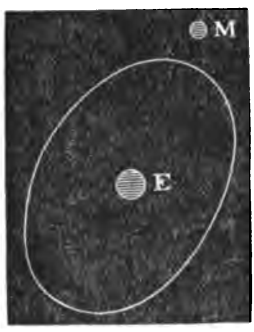
CASE 4. After the Last Quarter and before the New Moon, the Zodiacal Light of the evening is again faint, as it was before the First Quarter; *as though the illumination were wholly of that part of the girdle beyond the region near the longer axis.*



CASE 5. When the moon is nearly in Quadrature, *it would seem that the Zodiacal Light must appear short and bright, if apparent at all,* after the twilight of the evening; or before twilight in the morning. *For then the sun's light would be transmitted by a short course through the most curved, and probably more dense portions, near to one end of the longer axis of the oval.*

These are phenomena to which I invite special attention in the way of renewed and careful observation.





I should have said that in stating these five cases of appearances of the zodiacal light, I am contemplating the supposition that the girdle *encompasses* the moon.

IV.

ON SIR WILLIAM HERSCHEL'S OBSERVATIONS OF THE SATELLITES OF URANUS.

BY E. S. HOLDEN.

(READ 1874, JUNE 6.)

It is well known that Sir WILLIAM HERSCHEL suspected that the planet which he had discovered was accompanied by six satellites. These he numbered in their order proceeding outwards from the planet.

I.	Period	5	days,	21	hours.
II.	"	8	"	18	"
III.	"	10	"	23	"
IV.	"	13	"	11	"
V.	"	38	"	1	"
VI.	"	107	"	16	"

Of the existence of satellites II. and IV. there is no manner of doubt, since they were steadily observed by the elder Herschel from 1787, Jan. 11, to 1810, May 25, and by his son in the years 1828 to 1832, as well as by Lamont, Struve, Lassell, and Newcomb since that time, and all of these observations have been consistent.

Satellites I., III., V., and VI., have no such evidence in favor of their existence as the brighter ones II. and IV.

From the *Philosophical Transactions*, 1815, where Herschel has collected all his observations of the satellites of Uranus, it appears that he supposed satellite I. to have been observed on four different occasions, viz. :—

January 18, 1790.
March 27, 1794.
February 15, 1798.
April 17, 1801.

Satellite III. was seen only twice, on March 26 and 27, 1794, while V. and VI. were suspected at various times.

Lassell, in 1847, discovered two satellites interior to Herschel's II., and we owe to him and to his assistant, Marth, a good series of observations of all four satellites. The periods as determined by Lassell are—

Ariel :	2^d	12^h	29^m	$20^s.7$
Umbriel :	4	3	28	7.5
Titania :	8	16	56	25.6
Oberon :	13	11	6	55.4

It is evident that Herschel's II. and IV. are Lassell's Titania and Oberon.

Lassell's station at Malta was much better in regard to clearness of sky than Herschel's in England, his instrumental means were far superior, and the altitude of Uranus was greater at Malta in 1864 than in England in 1798, so that we must assume that if Lassell could not see Herschel's I., III., V., and VI., they did not exist.

In his report of his observations (Memoirs R. A. S., vol. 36) Lassell says that he repeatedly scrutinized the vicinity of the planet for the purpose of detecting faint satellites exterior to Oberon, and that he never suspected the existence of any such. Therefore, in the examination of Herschel's observations, I shall reject all those referring to satellites V. and VI., and for the same reason I shall reject all those referring to satellite III.; in this latter case we have the added testimony of five months' observations with the Alvan Clark refractor of the U. S. Naval Observatory at Washington.

There remain, then, of Herschel's observations only those of suspected interior satellites which it will be profitable to examine.

Before selecting any of these observations for discussion it is necessary to premise a few words in regard to Herschel's method of observation. On a very few occasions he was able to faintly illuminate the wires of his micrometer for a determination of the position of Oberon and Titania, but *all* of his estimations of the position of any small objects interior to these had to be made in a perfectly dark field. Hence these estimations are liable to a large error of from 5 to 15 degrees in position angle. Owing to the glare of the planet in the field of the telescope Herschel found that he could seldom see Oberon nearer to the planet than 23.6″, while Titania was usually invisible at distances less than 18.1″. Of course, under ordinary circumstances, Ariel and Umbriel could not be visible at all, but there were occasions when the fine polish of his mirror or the good state of the atmosphere permitted him to view objects even as close as 10″. It was evidently impossible for him to see an interior satellite on two consecutive nights, and of this he was fully aware.

It was his habit to make his observations of the "first" and "second" satellites (*i. e.* of Titania and Oberon), and to map down all small stars near to the planet. On the next subsequent observing night he examined the spot where the planet had been, and was thus able to identify all small stars as such. In his printed observations the sketches of star configurations are not given, but his remarks in full are quoted, followed by an "identification," as he calls it, of all suspected satellites. The patience and skill with which these identifications are carried out year after year are truly admirable, and they give a real value to that

which, without them, would be simply a ponderous mass of useless material.

I have selected from his observations as printed, *all* cases where he has seen an object interior to Titania sufficiently well to allow him to give an estimate of its position, excluding, of course, all cases where he has subsequently proved that such object was certainly a star.

The cases for examination are—

1. 1787, Feb. 10	Position	of interior	satellite	135°
2. 1790, Jan. 18	"	"	"	"following"
3. 1790, Jan. 20	"	"	"	315°
4. 1793, Feb. 5	"	"	"	250° 57
5. 1793, Mar. 9	"	"	"	205°
6. 1794, Feb. 28	"	"	"	66°
7. 1794, Mar. 27	"	"	"	
8. 1798, Feb. 15	"	"	"	5° 11′
9. 1801, Apr. 17	"	"	"	189° distance 18″

The elements which I have used are provisional ones derived by Prof. Newcomb from Lassell's Malta observations. They are amply adequate to the present inquiry.

From these elements I have computed the angle of position and distance of Umbriel and Ariel in each of the cases above set down, and compared these with Herschel's observations, as follows:—

1. "1787, Feb. 10, $8^h\ 57^m$. The first satellite is about 53° n. p.; $8^h\ 33^m$, * * * * a supposed third is about 45° s. f. In a little more than four hours I saw the satellites go on with the planet, and also in their orbits. * * * * No subsequent observation of the third was made."

On this date Umbriel was in P = 82° distant 16″ and Ariel was n. p. Hence Herschel's "supposed third" was neither of the inner satellites.

2. "1790, Jan. 18, $9^h\ 32^m$. There is a supposed third satellite about two diameters of the planet following, extremely faint and only seen by glimpses; $1^h\ 6^m$ after I could not perceive it; a fourth is about 70° n. p."

"Two diameters" of the planet was about 8″, and as Herschel usually counted his distances from the *limb* of the planet in his estimations, the distance from the centre would be about 10″. Umbriel was in P = 124° 23′, and distant 13″.9. Ariel was in P = 317° 10′, distant 10″.96. "$1^h\ 6^m$ after" Umbriel was in P = 118° 48′, and distant 13″.54; *i. e.*, *nearer* to Uranus by 0″.4. So far as the evidence goes we may reasonably infer that Herschel had a glimpse of Umbriel. In the *Phil. Trans.*, 1798, p. 271, Herschel, in referring to this observation, speaks of it as very certain, and supposes that the satellite might have been "11 or 12 degrees" from the parallel. The above identification, it is

true, brings it 34° from the parallel, but we must remember that in the first place the angle of position was merely an estimate, and secondly, that "following" is here shown by Herschel himself to have been but a rough term, not indicating an angle of position of exactly 90°.

The "fourth satellite" of this night was proved to have been a star.

3. "1790, Jan. 20, $12^h\ 5^m$ * * * * a third satellite is 45° n. p. in a line with the planet and second satellite."

Umbriel had a position angle of 301°, and was distant 13″.72; Ariel was n. f.; Oberon (the "second satellite") was in P = 324° 08′, and we must again conclude that Umbriel was seen.

4. "1793, Feb. 5, $9^h\ 18^m$ * * * * a very small star is 19° 3′ s. p. * * * * there is no subsequent observation of the small star."

Umbriel was in P = 187° 19′, and Ariel was n. p. Herschel's "small star" had a position angle of 250° 57′, and hence it was neither Ariel nor Umbriel.

5. "1793, March 9, $10^h\ 35^m$ * * * * a third (satellite) is about 65° s. p."

Umbriel was n. p. and Ariel was n. f., and hence this observation refers to neither of them.

6. "1794, Feb. 28, $9^h\ 43^m$. * * * * There is a small star * * * * about 24° n. f."

Both Ariel and Umbriel were n. p., and hence the small object was a star.

7. "1794, March 27. A supposed third of this evening is preceding the first satellite, but nearer the planet * * * * The first satellite was 79° n. f."

Titania (the "first satellite") was in P = 10° 20′, distant 34″.43, while Ariel was in P = 20° 48′, and distant 13″.48, Umbriel being at this time s. f. Hence we must conclude that Herschel saw Ariel.

8. "1798, Feb. 15, $12^h\ 13^m$ * * * * position of the supposed fifth satellite" (which was really Oberon) "84° 49′ n. f." * * * * at "about half the distance of the second satellite," and between it and the planet, Herschel saw what "must have been an interior satellite at its greatest northern elongation." Oberon was in P = 14° 46′, distant 32″.00; Ariel was in P = 213° 7′, distant 5″.06, and therefore invisible; Umbriel was in P = 194° 26′, distant 18″.99, and therefore in its most favorable position. Herschel says the interior satellite was between Oberon and the planet, and if this is so he did not see Umbriel. His account of this night's work (*op. cit.*, pp. 332–3 and 359) is confused, and leads to the suspicion (no more) that an examination of the originals might prove his position of the interior satellite 180° wrong—in which case Umbriel would have been seen. As it is we must suppose the contrary.

9. "1801, April 17, $10^h\ 30^m$. There is a third satellite at a great angle south preceding; in the configuration it is marked exactly in opposition to the second, and at half the distance of the first. * * * * the third by the configuration was 81° s. p."

On the next night Herschel examined the place where the planet was on April 17, and found no star in the former place of the third satellite. Herschel's satellite was in $P = 189°$, distant $18''$. Umbriel was in $P = 191° 27'$, distant $21''.18$. Hence Herschel saw Umbriel.

The above are all the cases which a careful examination of the printed observations suggests for discussion, and it is to be remarked that of the *four* cases where Herschel supposed he saw the interior satellites, *three* have been verified fully, and a reasonable suspicion exists that the fourth may have been likewise a veritable observation of Umbriel. A reference to the originals would probably settle most of the doubts which have arisen. We may conclude, then, that the elder Herschel was the first observer of Ariel and Umbriel, as well as of Titania and Oberon, but that he was unfortunately prevented from identifying the inner satellites because his telescope could not show them on two successive nights. It is to be noted that Sir John Herschel never caught a glimpse of them during his examination of Uranus with the same telescope in 1828 and 1832; the extreme difficulty of these objects makes us wonder at the marvellous skill and patience manifested by the elder Herschel in this difficult research.

It would be an interesting and useful research to endeavor to explain Herschel's observations of the III., V., and VI. satellites, and to show that these were observations of small stars. This research I hope to execute upon the return of Prof. Watson and Dr. C. H. F. Peters from their respective journeys for the purpose of observing the transit of Venus. Both of these astronomers have the most extended and minute maps of all the small stars in the region where Uranus was in 1787 to 1801; and a careful examination of tracings of these maps and of Herschel's observations could not fail to throw some light upon the supposed discovery of satellites III., V., and VI.

The next observations of these objects were by Lassell and O. Struve in 1847.

Lassell's observations are given in the *Monthly Notices of the Royal Astronomical Society*, vol. viii. p. 44. I have compared these with the theory as below.

	Date.	Lassell's position of Satellite.		Position of Umbriel.		Position of Ariel.	
		P	Δ	P	Δ	P	Δ
		°	″	°	″	°	″
1	1845, Oct. 5	324.1	...	Bet. 0 & 90	...	Bet. 0 & 180	...
2	1847, Sept. 14	350.	...	Bet. 0 & 90	...	345.4	13.96
3	1847, Sept. 27	326	...	...	...	320.1	10.45
4	1847, Sept. 29	336	...	173.3	...	1.3	13.27
5	1847, Oct. 1	348	18.44	354.8	19.10	...	...
6	1847, Nov. 6	80	10	141.6	15.4	...	...
7	1847, Nov. 6	349	11	...	...	350.1	13.89

Hence it is plain that Lassell saw Ariel on Sept. 14, Sept. 27, and Nov. 6, and *possibly* on Sept. 29. Umoriel was seen on Oct. 1.

Struve's observations are given in *Monthly Notices of the Royal Astronomical Society*, vol. viii. p. 46, as follows:—

	Date.	Umbriel (Computed).		Umbriel (Observed).
		°	″	° ″
1	1847, Nov. 1	P = 194.0	= 17.8	P = 186.47 Δ = 16.59
2	1847, Nov. 28	203.6	17.0	Between 0° and 90°
3	1847, Dec. 9	218.6	13.7	P = 152° 54′ Δ = 7″.4, while Ariel was between 270° and 360°.
4	1847, Dec. 10	180.1	17.0	P = 169° 38′ Δ = 19″.85
5	1848, Jan. 25	202.0	18″.0	Between 0° and 90°

Hence Struve may have seen Umbriel Nov. 1 and Dec. 10, 1847.

In 1871 observations of Ariel and Umbriel were made at Bothkamp with a telescope of twelve inches aperture and compared with Marth's ephemeris for that year. The positions of Ariel differ from their predicted positions by a large angle, nearly 180°, while the positions of Umbriel agree well. Ariel, however, is much the brighter of the two inner satellites, and as it evidently was not seen at all, it becomes probable that a small star was mistaken for Umbriel on the five nights of observation.

This supposition is strengthened when we consider that the Bothkamp observers found Titania and Oberon difficult objects, which they certainly are not to any telescope which will show Ariel or Umbriel. Lassell estimates that Oberon and Titania are twice as bright intrinsically as either of the inner satellites, and this estimate is probably not too high.

We may then fairly claim that Sir William Herschel saw all four of the satellites of Uranus, that Lassell discovered independently

and before any of his contemporaries the two faint satellites, while Struve probably saw Umbriel on one or two occasions, and that these inner satellites have not all been seen with any telescopes save the twenty and forty feet reflectors of Herschel, the telescopes of Mr. Lassell (two and four feet reflectors) and by the Clark refractor at Washington.

The great reflecting telescope of Dr. Henry Draper, and the Clark refractor at Chicago, and Mr. Newall's Cooke refractor, have never been used upon these objects so far as I know, although all three are undoubtedly adequate to the purpose.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JUNE, 1874.

*** Absent or removed from Washington. † Deceased.**

CLEVELAND ABBE
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BENJAMIN ALVORD
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HENRY HOBART BATES
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George M. Wheeler
*Junius B. Wheeler
A. D. Wilson
James Ormond Wilson
William Maxwell Wood
Joseph Janvier Woodward
John Maynard Woodworth

Mordecai Yarnall
Henry Cripsey Yarrow

INDEX TO NAMES OF CONTRIBUTORS.

Joseph Henry

SMITHSONIAN INSTITUTION

BULLETIN

of the

PHILOSOPHICAL SOCIETY

of

WASHINGTON.

VOL. II.

[11]

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. II.

OCTOBER 10TH, 1874—NOVEMBER 2D, 1878.

(With a Portrait and two Lithographs.)

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON.

1875—1880.

CONSTITUTION, STANDING RULES,

AND

LIST OF MEMBERS

OF

THE PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

December, 1875.

CONSTITUTION

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all business.

ARTICLE VI. This Constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

DECEMBER, 1875.

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. The Annual Meeting for the election of officers shall be the first stated meeting in the month of November. When necessary, Special Meetings may be called by the President.

3. Notices of the time and place of meetings shall be sent to each member by one of the Secretaries.

4. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

5. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

6. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

7. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

STANDING RULES

OF THE

GENERAL COMMITTEE

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

DECEMBER, 1875.

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half past seven o'clock on the evening of each stated meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee and of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. Proposals of new members may be read at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concurrence of twelve of the members shall be necessary to election.

8. These Standing Rules, and those for the government of the Society, shall only be modified with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

1. The President's annual address will be published in full.

2. When directed by the General Committee, any communication may be published in full in an appendix to each volume.

3. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

4. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with reference to the place of publication, if made known in season to the Committee on Publications.

NOTE. *The attention of members to the above rules is specially requested.*

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

1875, '6.

President,	JOSEPH HENRY.
Vice-Presidents,	J. K. BARNES, WM. B. TAYLOR, J. E. HILGARD, J. C. WELLING.
Treasurer,	PETER PARKER.
Secretaries,	J. H. C. COFFIN, T. N. GILL.

MEMBERS OF THE GENERAL COMMITTEE.

C. ABBE, N. S. LINCOLN,
S. F. BAIRD, O. M. POE,
C. E. DUTTON, S. NEWCOMB,
E. B. ELLIOTT, C. A. SCHOTT,
J. J. WOODWARD.

STANDING COMMITTEES.

ON COMMUNICATIONS:

Chairman, J. J. WOODWARD;
J. E. HILGARD.

ON PUBLICATIONS:

Chairman, S. F. BAIRD;
J. H. C. COFFIN, T. N. GILL, C. ABBE.

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BULLETIN

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

73D MEETING. OCTOBER 10, 1874.

Vice-President W. B. TAYLOR in the Chair.

Mr. THEODORE GILL read a paper

ON THE PRODROMUS METHODI MAMMALIUM OF STORR.

(Published in full in Appendix V. of this Bulletin.)

Mr. E. B. ELLIOTT made a communication

ON THE USE OF METRIC WEIGHTS AND BALANCES FOR POSTAL PURPOSES IN THE UNITED STATES:

calling attention to the fact that fifteen grammes, a weight but slightly greater than one-half ounce avoirdupois, had been made by statute law its equivalent for postal purposes.

Dr. B. A. GOULD, Director of the Argentine National Observatory at Cordoba, was then introduced, and, in answer to several questions, gave an account of the general condition of scientific culture in the Argentine Republic.

74TH MEETING. OCTOBER 24, 1874.

The President, Mr. JOSEPH HENRY, in the Chair.

Rev. Dr. C. W. SHIELDS, of the College of New Jersey, Princeton, read a paper

ON THE PRESENT STATE OF THE SCIENCES:

depicting the development and bounds of scientific research, and attempting to show that there is no need of any serious differences between science and religion.

(*See New York Tribune, Nov.* 6, 1874; *also a volume published by the author.*)

The thanks of the Society were presented to Dr. SHIELDS for this essay, and the hope expressed that it would be widely circulated.

Mr. S. C. BUSEY made a communication

ON THE GATHERING, PACKING, TRANSPORTATION, AND EXPOSURE OF FRUITS FOR SALE:

giving a summary of an extensive investigation into the causes which lead to the deterioration of the vegetable foods as commonly used, with some reference to diseases resulting therefrom.

75TH MEETING; FOURTH ANNUAL MEETING. NOV. 7, 1874.

Vice-President J. E. HILGARD in the Chair.

Twenty-nine members present.

The order of proceedings for the evening was announced, and the following officers of the Society were elected for the ensuing year:—

President,	JOSEPH HENRY.
Vice-Presidents,	J. K. BARNES, M. C. MEIGS,
	J. E. HILGARD, WM. B. TAYLOR.
Treasurer,	PETER PARKER.
Secretaries,	J. H. C. COFFIN, T. N. GILL.

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C. E. DUTTON,	O. M. POE,
E. B. ELLIOTT,	C. A. SCHOTT,
N. S. LINCOLN,	J. C. WELLING,

J. J. WOODWARD.

The following members have been elected during the year :—

Frederic A. Sawyer,	James W. Milner,
Orlando M. Poe,	Henry Yarrow,
Isaiah Hanscom,	Archibald R. Marvine,
Henry H. C. Dunwoody,	Edwin E. Howell,
William M. Mew,	John M. Woodworth,
Leonard D. Gale,	Ormond Stone,
Henry R. Rathbone,	Josiah Curtis,
Elliott Coues,	John Stearns,
Frederic W. Dorr,	Edward O. Graves,
Hugh Ewing,	Frank W. Clarke,
Charles Ewing,	John W. Chickering,
James T. Gardner,	Henry Garnett,
John W. Powell,	Henry W. Henshaw,
John Sherman,	A. D. Wilson,
Samuel C. Busey,	A. C. Peale,
William Lee,	David D. Porter,
Charles Henry Davis,	Charles Warren,
George B. Goode,	John Eaton,
Robert Ridgway,	John J. Knox.

Mr. J. W. Chickering read a paper

ON THE CORRELATION OF THE WINDS AND THE TEMPERATURES OF THE SURFACE WATERS OF THE OCEAN ALONG THE COAST OF NEW HAMPSHIRE.

(ABSTRACT.)

During the years 1870–'74, between the 16th July and the 15th August, a series of observations has been made at Hampton Beach, N. H., numbering 98 in all, and including temperature of air and water, direction and force of wind, and state of tide. The extremes of surface temperature have been 52° and 72° Fahr.

19 observations gave a temperature above 65°; 12 gave a temperature below 55°, of which 11 were during 1874.

All the high temperatures were noted in connection with winds from the sea; all the low temperatures with land winds. The averages for each year were as follows :—

1870,	61°
1871,	63°
1872,	61°
1873,	64°
1874,	57° +
1870–1874,	60° +

Many changes were noted, of which the following is a specimen:—

July 24th,	E. wind,	72°
" 26th,	S. W. "	56°
" 26th,	E. "	65°

One very rapid change occurred:—

July 22d,	6 A. M.	N.W. wind,	54°
" "	12 M	S. E. "	65°

As a general result, these changes in temperature followed changes in wind within three or four hours.

A few exceptions were noted, where the rise in temperature preceded the coming of the easterly wind by two or three hours.

The most satisfactory theory of this correlation is—that with a strong wind off shore, the warmer surface waters are driven off and replaced by the colder waters from beneath; and, with a sea-breeze, the process is reversed.

Some of these exceptions remain to be harmonized.

Several series of observations of temperatures at different depths were taken, of which one will serve as a sample:—

July 31. Four miles E. from Boar's Head—

17 fathoms,	42½°
15 "	43°
10 "	44½°
5 "	47°
4 "	49°
Surface,	53°

Mr. Dutton, among other remarks, suggested that hygrometrical observations were desirable, and might aid in the solution of cooler winds coincident with warmer water.

Mr. Abbe remarked that he found the anomalous cases quite interesting, those, namely, concerning which Professor Chickering had stated that the change in the temperature of the water was observed a few hours before the change in the wind occurred. He did not think that we needed to have recourse to any occult influence of the barometer, moisture, etc., but that probably the simpler explanation would be found in the fact that over the sea the wind had actually changed, but that its influence had not yet

been felt upon the land where the observer was situated. He alluded to the fact that frequently vessels are seen a short distance from land enjoying winds very different from those prevailing on shore ; and further illustrated the subject by explaining the local winds observed on either side of the Great Lakes, shown on the Signal Service maps.

The President spoke of the desirableness that those accustomed to scientific research should note phenomena around them, even at places resorted to for recreation.

Mr. E. B. Elliott presented

FURTHER REMARKS ON METRIC WEIGHTS AND BALANCES FOR THE POSTAL SERVICE:

stating that efforts were making, which it was hoped would prove successful, to have the law, which recognized 15 grammes as the equivalent of the half-ounce avoirdupois for postal purposes, applicable not merely for international purposes, but for all purposes—domestic as well as international.

77th Meeting. December 5, 1874.

The President in the Chair.

Forty-three members and visitors present.

Mr. H. H. Bates read a paper

ON THE MOVEMENT OF A PARTICLE ATTRACTED TOWARDS A POINT.

(ABSTRACT.)

In this paper it was shown that the ambiguous conclusions heretofore arrived at, in the analytical discussion of the movement of a particle attracted towards a point, involving such absurdities as infinite attractive force and infinite velocity, were due to the tacit assumption of want of magnitude in the attracted particle. Said particle, however minute, must be regarded, relatively to a simple attracting point, as a mass or sphere. But Newton has shown that the point of maximum attraction in a homogeneous sphere is not at its centre, but at its surface. The law of attractive force, therefore, changes, when the attracting point penetrates the surface, and, instead of being inversely as the square of the distance, becomes directly as the distance ; that is, a diminishing force, reaching zero at the centre, instead of

infinity, with the corollary of finite velocity, passing by continuity from an accelerated into a retarded velocity as the centre of the particle passes the attracting point.

MR. J. J. WOODWARD read a paper

ON THE SIMILARITY BETWEEN THE RED BLOOD-CORPUSCLES OF MAN AND THOSE OF CERTAIN OTHER MAMMALS, ESPECIALLY THE DOG; CONSIDERED IN CONNECTION WITH THE DIAGNOSIS OF BLOOD-STAINS IN CRIMINAL CASES.

(*Published in full in the American Journal of the Medical Sciences, January*, 1875.)

(ABSTRACT.)

The writer, after deprecating the mischievous tendency of a recent paper "*On the value of high powers in the diagnosis of blood-stains*," shows that the common supposition that a certain small but constant difference exists between the average diameter of the blood-corpuscles of man and the dog, which might serve as the basis of a distinction in legal cases, is not borne out by an examination of the original papers of Gulliver and Welcker, whose measurements are those vulgarly relied upon in favor of this view, and also that it is not in accordance with the actual facts of the case. He then details the precautions which should be adopted to secure accuracy in such measurements, and gives the following statement of the results of his own measurements of the blood of five men and five dogs.

Measurements of Human Red Blood-Corpuscles from five Individuals.

	No. of corpuscles measured.	MEAN DIAMETER.	
		Decimals of an English inch.	Decimals of a millimetre.
1. Dr. W. dry	50	.000304	.00772
2. " " moist	49	.000292	.00742
3. " " " (H.) . .	50	.000300	.00762
4. " " " (H.) . .	50	.000289	.00734
5. Dr. McC. dry	50	.000288	.00731
6. " " "	50	.000294	.00747
7. " " moist	50	.000301	.00765
8. Mr. W. dry	50	.000298	.00757
9. " " " (H.) . .	52	.000297	.00754
10. Mr. T. "	50	.000290	.00737
11. " " " (H.) . .	50	.000292	.00742
12. Mr. B. "	50	.000296	.00752
13. " " " (H.) . .	50	.000297	.00754

In each of these measurements of human blood, the great majority of the corpuscles ranged from twelve to seventeen divisions of the eye-piece micrometer; that is, from .00024 to .00034 of an inch. Out of the whole number measured, six were as small as ten divisions, and one as large as eighteen divisions; large and small forms were not searched for, however. The size most frequently measured was fifteen divisions, or .00030 of an inch.

Examination of Red Blood-Corpuscles of the Dog from five Individuals.

	No. of corpuscles measured.	Mean Diameter.	
		Decimals of an English inch.	Decimals of a millimetre.
1. Mongrel terrier, dry . .	50	.000292	.00742
2. Same animal, " . .	54	.000299	.00759
3. Another mongrel terrier, dry (H.)	50	.000290	.00737
4. Same animal, moist . (H.)	50	.000288	.00731
5. Scotch terrier, " . (H.)	50	.000291	.00739
6. Same animal, " . (H.)	50	.000289	.00734
7. " " " . (H.)	49	.000287	.00729
8. Spitz dog, dry . (H.)	52	.000285	.00724
9. Black and tan, moist . (H.)	50	.000290	.00737

In each of these measurements of dogs' blood, precisely as in the case of those of human blood, the great majority of the corpuscles measured from twelve to seventeen divisions of the eye-piece micrometer (.00024 to .00034 of an inch). Out of the whole number measured, four were as small as ten divisions, but none larger than seventeen were encountered. As with the human blood, however, large and small forms were not searched for, but all the perfectly formed corpuscles brought into view by the movement of the stage, were measured as they passed under the micrometer without selection until the required number was recorded. The size most frquently measured was fifteen divisions, or .00030 of an inch, precisely as in the case of human blood.

It will be observed that three of the above means for human blood, Nos. 1, 3, and 7, are a trifle larger than any of those of dogs' blood, and two of the latter, Nos. 7 and 8, are a trifle smaller than any of those for human blood. All the other means for the dog are within the range of the values found for human blood, and the majority of them are each identical, even to the last decimal place, with some one of those found for man.

The author has not made systematic measurements of the blood of other animals besides the dog, whose blood could not be dis-

tinguished from that of man, but he points out that the measurements of Mr. Gulliver himself warrant the belief that the blood-corpuscles of the rabbit and guinea-pig, among domestic animals, besides those of most of the monkeys of both the old and new world, the seal, the otter, the kangaroo, the capybara, the wombat, and the porpoise, belong to the same category. The paper contains full references to the literature of the microscopical diagnosis of blood-stains, and terminates with the following paragraph :—

"In conclusion, then, if the microscopist, summoned as a scientific expert to examine a suspected blood-stain, should succeed in soaking out the corpuscles in such a way as to enable him to recognize them to be circular disks, and to measure them, and should he then find their diameter comes within the limits possible for human blood, his duty in the present state of our knowledge is clear. He must of course, in his evidence, present the facts as actually observed, but it is not justifiable for him to stop here. He has no right to conclude his testimony without making it clearly understood by both judge and jury, that blood from the dog and several other animals would give stains possessing the same properties, and that neither by the microscope nor by any other means yet known to science, can the expert determine that a given stain is composed of human blood, and could not have been derived from any other source. This course is imperatively demanded of him by common honesty, without which scientific experts become more dangerous to society than the very criminals they are called upon to convict."

Mr. Ormond Stone read a paper

ON THE CORRECTION OF A COMET'S ORBIT :

showing that some of the ordinary formulæ could be simplified in practice.

(*This paper is published in full in the Astronomische Nachrichten, No.* 2023.)

Mr. Joseph Henry made a communication

ON AUDITION.

Remarks were made by Messrs. Dutton, Hilgard, Meigs, and Eastman, chiefly on the sound produced by the discharge of guns, and the regurgitation ; Mr. Woodward following with a description of the horny fibres of the human ear, which vibrated at different sounds.

78TH MEETING. DECEMBER 19, 1874.

The President in the Chair.

Forty-three members and visitors present.

Mr. J. T. GARDNER gave an extended abstract of a paper, prepared by him for the Report of the Geological Survey of the Territories for 1873,

ON THE USE OF RAILROAD LEVELLINGS IN DETERMINING ELEVATIONS ON THE GREAT LAKES AND RIVERS IN THE UNITED STATES AND IN THE ROCKY MOUNTAINS.

(ABSTRACT.)

In connection with the work of the Geological and Geographical Survey of the Territories under Prof. Hayden, Mr. Jas. T. Gardner, the geographer of the survey, has undertaken to review the evidence upon which rests the received elevations of the principal points in the United States. The results are published in the Report of Prof. Hayden for 1873.

Mr. Gardner had before him over 1200 railroad and canal profiles, collected in Washington by different departments of the government. He also visited the offices of the leading railroads to examine original notes and ascertain exact details at the termini and intersection of roads, so that the profiles of the different lines might be accurately corrected.

It was found that the old elevations given by our best authorities had two leading sources of error. The eastern ends of main railroads and canals had never been properly connected with tide gauges, so placed as to give the mean level of the ocean; and the old reports of railroad and canal heights had in many cases been superseded by recent and more accurate levellings. Having sifted his great mass of data to retain only the most trustworthy of the lines, and carefully connecting these with the U. S. Coast Survey tide gauges, Mr. Gardner proceeded to determine the elevations of our principal raiiroad centres from as many independent lines as possible.

At Cleveland there were three results, which only differed among themselves one foot; and the five separate results for the surface of Lake Erie differ only 2½ feet.

The elevations of Lakes Huron and Michigan are determined by nine wholly or partially independent lines, which differ only four feet. This agreement of results places the elevations of our lakes beyond doubt.

Lake Erie's mean surface is 573 08 feet, and Lakes Michigan and Huron are 589.15 feet above the sea. In the same way

points along all great rivers were located, and cities at the foot of the Rocky Mountains.

The general result of the investigation is to show that we did not know accurately the elevation of any points except on our seaboard. The elevation of our lakes is changed by it; the fall of the Ohio, Mississippi, and Missouri Rivers is materially altered; and the mean surface of the continent is found to be higher above the ocean than was supposed.

The Saint Louis directrix is fixed at 428.29 feet, a change of 23 feet from the old determination; while Kansas City and all the railroads leading westward from it are shown to have an error of 115 feet. One of the most striking instances of accuracy in long lines of American railroad levels is given by connecting the New York Central and Lake Shore and Michigan Southern Roads to Chicago, and thence southward by the Illinois Central and other roads to New Orleans, making a line 1800 miles long, which reaches the gulf with an error of only 2½ feet.

By the Iowa railroads and the Union Pacific R. R., the elevation of Denver is deduced from Chicago; and by the Missouri roads and Kansas Pacific, the height of Denver is gotten from Saint Louis. These independent results differ less than five feet, and the elevation of Denver is established at 5196.58 feet. The heights of the principal Rocky Mountain peaks were measured above Denver; and now, that this point is fixed, we can know with certainty the elevations of our great mountains.

Considerable discussion followed, mainly on the effect of local attractions in determinations by the spirit-level on mountain slopes, in which Messrs. HILGARD, ABBE, GARDNER, and CUTTS participated.

Mr. ABBE said that, for the sake of clearness, he would state the hypsometric problem as it presented itself to him.

We desire to deduce the exact figure of the earth's surface, or the position of each point of its surface, relative to a system of co-ordinate axes, whose origin is an assumed approximate centre. To this end we assume for the whole earth Bessel's, or some other, ellipsoid, that agrees well with a limited known portion, and must then determine, as deviations from this geometrical figure, the irregularities of the actual surface.

If by levelling operations we seek to determine the relative elevations of the surface, we obtain thereby the altitudes not above the assumed spheroid, but above an irregular level surface which is the result of the irregularities in the direction of gravity.

This level surface is that in which civil engineers are interested, but not that which the higher geodesy seeks to determine, nor that which astronomers would use if, as in the works involving parallax, the exact distance of a station were required from the assumed centre of the earth.

If we would determine the altitudes above the adopted normal ellipsoid, our levellings require indeed a correction for "local attraction of the plumb line," which, as Baeyer has shown for Germany (*Astron. Nach.*, No. 1993), may amount to many feet in the case of our western plateaus.

The President in this connection gave some historical reminiscences of early surveys for the Erie canal.

79TH MEETING. JANUARY 2, 1875.

Vice-President TAYLOR in the Chair.

Eight members present.

Mr. J. J. WOODWARD made remarks

ON THE MODERN MICROSCOPE, NOBERT'S LINES, AND THE ATTEMPTS OF OTHERS TO CONSTRUCT THEM;

followed by a conversational discussion extending to the theory of vision and the structure of the human retina, in which Messrs. TAYLOR, SKINNER, and WOODWARD participated.

Mr. E. B. ELLIOTT made remarks

ON THE TRANSITION IN GERMANY, AND THE SCANDINAVIAN NATIONS OF SWEDEN, NORWAY, AND DENMARK, FROM THE SILVER STANDARD OF COINAGE AND MONEY OF ACCOUNT TO A GOLD STANDARD.

Adjourned MEETING. JANUARY 9, 1875.

The President in the Chair

Twenty-two members and visitors present

Mr. L. D. GALE made a communication

ON THE FAILURE OF THE WOODEN PAVEMENTS OF WASHINGTON CITY:

contrasting them with those of other cities, and attributing their decay in great part to the practice of sprinkling the streets. He also described a contrivance for protecting them, which he had invented.

Mr. T. N. GILL made a communication

ON THE GEOGRAPHICAL DISTRIBUTION OF MAMMALS:

and elucidated doubtful points by reference to phenomena in other classes of vertebrates. His conclusions were that at remote periods Australia, South America, and Africa had been colonized from a common source, and hence might be grouped into a division—*Eogaea*—contrasted with another—*Pleiogaea*—containing other regions. Of these Australia retains the greater number of primitive features, as illustrated by Paleontology; and Africa has received the greatest number of intrusive elements.

Considerable discussion followed, in which the President and Messrs. MEIGS, WELLING, and TAYLOR participated.

80TH MEETING. JANUARY 16, 1875.

Vice-President TAYLOR in the Chair.

Forty members and visitors present.

The Chair announced the election of Dr. A. F. A. KING, Dr. EMIL BESSELS, and Mr. JOSEPH WOOD as members of the Society.

Mr. C. E. DUTTON read a paper

ON THE GLACIAL PERIOD:

giving an historical account of the progress of speculation and investigation, and the more recent theories respecting such a period, and showing the slight basis on which they rested, and stating some facts and considerations opposed to them. He also referred to a recent article by Mr. Sclater on the geographical distribution of animals.

Mr. GILL followed, remarking on the combinations of the several faunas; and dissenting from the views of Mr. Sclater, as stated by Mr. DUTTON. He likewise discussed the extension of warm-water forms northward in the preglacial epoch, and the extension of cold-water forms southward in the glacial epoch.

Gen. G. K. WARREN, U. S. Engineer, by request, followed with remarks on changes in the interior section of North America, stating that, at some time subsequent to the glacial period, Lake Winnipeg drained to the south, instead of the north, as at present; and that the northern portion of this region has been depressed.

Mr. GALE made remarks on the heaps of bowlders, arranged in lines across Manhattan Island and Long Island, passing over ranges of hills; expressing the opinion that these bowlders must have come from some region more than forty miles distant.

Mr. TAYLOR brought up CROLL's theory, that when the eccentricity of the earth's orbit was at its maximum, a coincidence of the aphelion and winter solstice would greatly increase the cold of northern winters, and thus might be sufficient to produce a glacial period.

Mr. ABBE remarked on the insufficiency of this to account for the great increase of cold.

Mr. DALL gave an account of what he had observed of very great elevations and depressions, at different periods, in the Aleutian Isles and on the Yukon in Alaska.

81ST MEETING. JANUARY 30, 1875.

The President in the Chair.

Fifty members and visitors present.

The President announced the election of Hon. GEORGE BANCROFT, Dr. ANTON ZUMBROOK, Prof O. T. MASON, Col. S. THAYER ABERT, Col. GARRICK MALLERY, and Lieut. HENRY JACKSON as members of the Society.

Mr. E. S. HOLDEN read a paper

ON THE NUMBER OF WORDS USED IN SPEAKING AND WRITING.

(*This paper is published in Appendix VI. of this Bulletin.*)

Mr. GILBERT remarked on the propriety of counting only independent words, and excluding mere inflections; and counting in this way he had estimated his own vocabulary at from 10,000 to 14,000 such words.

Mr. PARKER referred to the Chinese language, stating that the Bible in that language required only 3600 characters, and that 1000 characters sufficed for common use.

Mr. HILGARD stated that only 600 words were to be found in Italian operas; and remarked that Mr. MARSH probably excluded inflections, and estimated only words which educated men would usually employ, not such as would be used on special occasions.

Mr. POWELL spoke of the language of the Utes, as comprising about 1600 roots and 8000 words.

Mr. EASTMAN urged the importance of classifying words according to the frequency with which they are used.

Mr. GILL remarked that 30,000 did not seem to be too large an estimate of the number of words used by an educated man; and gave an estimate of the number of technical terms which a naturalist must have at ready command. A zoologist required about 25,000.

Mr. KNIGHT followed on the number of words in a technical work of his own on Mechanics; and Mr. FARQUHAR on the multiplication of words resulting from grammatical variations.

Mr. WELLING remarked that the variations in such estimates were wide, and suggested that it was necessary to consider separately what words would, could, or might be used, and that the investigation should be based on what had been done, not on what might be done.

Mr. J. E. HILGARD made remarks

ON A PROPOSED REFORMATION OF THE GREGORIAN CALENDAR:

in a Bill recently introduced in the Congress of the United States. It proposes to make the year commence at the winter solstice, April at the vernal equinox, July at the summer solstice, and October at the autumnal equinox. Thus the arrangement of the civil year would correspond with these four astronomical epochs.

Mr. COFFIN remarked that the year commencing on the 1st of January, with months of different lengths and an intercalary day in February each fourth year, had come down to us from Julius Cæsar. In the time of Augustus one day taken from some other month was added to August, in compliment to the reigning Emperor. An important change was made in 1582, under the authority of Pope Gregory XIII., making the civil year more nearly coincident with the tropical year, and, by dropping 10 days in October of that year, restoring the vernal equinox to the 21st of March, its date in the year 325, the time of the Council of Nice, whose regulation of the ecclesiastical calendar depended on that day. The inconveniences, difficulties, and delays in effecting that change are well known. It was not adopted in Great Britain and the American Colonies until towards the middle of the last century. In Russia at the present day the *Old Style*, as it is called, is still retained.

From the different modes of reckoning the year in different countries and at different periods, astronomers adopt the day as the unit, and by giving the *Julian day* of the commencement of each year, whatever its *Style*, are able to determine the dates of past phenomena or chronology.

The alteration proposed not only changes the time of the commencement of each month, but involves also other changes in some of them. The difficulties of the past would follow any new change, and the one proposed presents too limited advantages to compensate for them.

The true vernal equinox occurs at present on the 20th of March in Washington time, also in European time except in each year preceding a leap year. At the beginning of the next century it will be restored to the 21st.

Mr. E. B. Elliott spoke of the desirableness of a change in the calendar, but not in the way proposed; and of the great advantages in having the months 30 and 31 days in length alternately, and putting the intercalary day at the end of the year instead of February.

The simplification of certain astronomical tables by this last change was referred to by Mr. Stone.

Mr. Parker remarked that in China four new year's days occurred within a few days of each other without serious inconveniences—those of Zoroaster, the Jews, the English, and the Russians.

Mr. Taylor objected to the change proposed in the bill before Congress, and described at length a year commencing with the vernal equinox with months alternately of 31 and 30 days, dropping one day at the end of each common year, advocating it as a desirable change, could it be effected.

Mr. Hilgard read part of an article in the Encyclopædia Britannica on the calendar, and of the British Statute of 1751, adopting the Gregorian calendar "for that portion of Great Britain called England;" remarking that the year at that time in England commenced on the 25th of March, and in Scotland on a different day.

He remarked in substance that the conclusion generally arrived at appeared to be that the calendar could be greatly improved—

1st. By beginning the year at the winter solstice.

2d. By having months alternately of 30 and 31 days, the last month having 31 days in leap years, 30 days in common years.

3d By placing the intercalary day at the end of the year, instead of the second month, but otherwise adhering to the Gregorian mode of intercalation.

The change should be made, if made at all, at the commencement of the next century. But no one undertakes to say that the advantages gained would balance the inconveniences arising from such a change.

82D MEETING. FEBRUARY 13, 1875.

The President in the Chair.

Forty-three members and visitors present.

Mr. F. M. ENDLICH read a paper

ON THE COLORING AGENT OF GEMS:

giving analyses of the coloring matter of a great variety of gems and sub-gems, describing in several cases the different changes produced by heating in the open air and when air is excluded, and concluding that iron, chrome, and manganese are the principal agents in giving color to these minerals.

Mr. ASAPH HALL, on behalf of Rear-Admiral DAVIS, President of the Commission on the transit of Venus, communicated letters

ON THE OPERATIONS OF THE SEVERAL PARTIES SENT FROM THE UNITED STATES TO OBSERVE THE TRANSIT OF VENUS ON THE 8TH OF DECEMBER, 1874.

(ABSTRACT.)

Dr. C. H. F. PETERS writes from Queenstown, Otago in New Zealand, that he observed with the equatorial the first and second contacts: the former uncertain, the latter with great precision; none of the much talked of phenomena presenting themselves to his eye. 178 photographs were taken in the interval of these contacts, and 59 while the planet was in the disk. The sun was out almost uninterruptedly during the first 1¾ hours. Then came clouds, with small intervals of sunshine. The last photograph was taken 10 minutes before the beginning of egress, and from that time the sun was under a dense cloud, so that the egress was lost. With this exception, the observation of the transit has been successfully accomplished. At all other stations in New Zealand observations were prevented by clouds and rain. The American party escaped disappointment by being at a greater elevation above the sea.

Capt. CHARLES W. RAYMOND, U. S Engineer, gives a full account of his arrangements for observing the transit at Campbell Town, Tasmania. Heavy clouds and rain prevented observations of the 1st, 2d, and 4th contacts. The 3d was observed with the equatorial, light clouds drifting over the sun and planet. The planet seemed to gradually assume the pear-shape. No shooting out of the planet towards the sun's limb at or near

contact was observed. Quite a number of measurements of distances were made during a cessation of the storm, the sun appearing at intervals through the clouds. 55 full-sized photographs were taken while the planet was on the disk of the sun, and 77 with the Janssen apparatus between the 3d and 4th contacts.

Prof. HARKNESS reports that at Hobart Town bad weather prevented observations of contacts, but many photographs were taken.

Mr. GEORGE DAVIDSON, of the U. S. Coast Survey, at Nagasaki in Japan, obtained photographs with the Janssen apparatus to within 10^s or 15^s of the actual time of the 1st contact; then the clouds thickened, and when it brightened again the planet was 10^s, possibly 15^s, on the limb of the sun. Measurements of cusp-distances were made when the planet was half on the disk of the sun, until near the time of the 2d contact. The 2d contact was observed with the equatorial, and by Mr. TITTMAN with a smaller telescope, but through clouds. At this and the 3d contact there were very slight disturbances, no ligament, no band or black drop, no distortion. Good micrometric measurements were made of the separation of the limbs and of the horizontal diameter of Venus. The meridian transits of both bodies were observed with the transit instrument, and at the same time 1 micrometric readings were made by Mr. Tittman with another instrument for the difference of declination of the upper limbs of the sun and Venus.

Clouds covered the sun about 10^s before and cleared off 5^s after the 3d contact, with the minute cusps of the sun almost touching and sharply defined; clouds and rain followed.

About 60 good photographs were taken.

There was no part of the day when the sun was wholly unobscured, and the work was often interrupted by clouds. Of these there were two strata: the upper moderately thin, of cirrus and cirro-stratus, and persistent; the lower heavy and dense, of cumuli stratus. When the sun was seen it was through breaks in this stratum.

At Wladiwostok, Professor HALL reports that the buildings were finished and the instruments all mounted by the middle of October. The final adjustment of the photographic apparatus was made and regular practice in photography was begun Nov. 8th. The latitude of the station, $+43° 6' 35.7''$, was determined by the American method. The difference in longitude between Wladiwostok and Nagasaki, Japan, was determined early in November. The values of the magnetic constants were observed about the same time.

On Dec. 9th, everything was in readiness for observing the transit of Venus. The 1st and 2d contacts of the planet were

observed by Professor Hall with the five-inch equatorial, and by Mr. O. B. Wheeler with the three-inch equatorial. At the time of contacts the sun was covered with a haze, but the sun and planet were remarkably steady and well defined. The time of the third contact was observed by Mr. Wheeler with a good deal of uncertainty; and with the higher power used by Prof. Hall, the uncertainty was so great, on account of the faintness of the objects, that he did not record any time. The last contact was entirely lost on account of clouds.

It had been found in the preliminary practice that the glass mirror reflected so little light that, even on clear days, the time of exposure had to be increased so much, in order to get dense photographs, that these photographs were apt to be blurred and of indistinct outline. It was decided, therefore, to make fainter photographs with sharp edges. On the day of transit the haze added very much to the difficulty of photographing, and it was only when the haze lifted and the sun shone out brightly that photographs could be made. Of these 13 are very good.

Mr. HILGARD followed with remarks on the measurement of photographs of the sun, and expressed apprehension that the want of sharply defined outlines would not allow of their being measured with precision.

83D MEETING. FEBRUARY 27, 1875.

Vice-President HILGARD in the Chair.

Thirty-seven members and visitors present.

Mr. HILGARD read a paper—by Mr. M. C. MEIGS—

ON THE MOVEMENTS CAUSED IN LARGE ICE-FIELDS BY EXPANSION AND CONTRACTION, AS ILLUSTRATIVE OF THE FORMATION OF ANTICLINAL AND SYNCLINAL AXES IN GEOLOGICAL FORMATIONS.

(*This paper is published in Appendix VII. of this Bulletin.*)

Prof. C. A. YOUNG, of Dartmouth College, by invitation, gave an account of

THE EXPEDITION TO PEKIN FOR OBSERVING THE LATE TRANSIT OF VENUS,

under the direction of Prof. J. C. WATSON. He stated that on

the whole the expedition was successful, the contacts at ingress and egress having been observed, and a number of intermediate measurements made. On account of cloudiness of the weather, Prof. Young was unable to make observations with the spectroscope.

Mr. J. W. Powell read a communication

ON THE UINTAH MOUNTAINS,

in which he offered an explanation of the formation of the cañons in the southwest.

Prof. E. M. Gallaudet,
Com'r Lester A. Beardslee, U. S. N.,
Mr. A. N. Skinner,
Mr. Robert Lawrence Packard, and
Lieut. C. C. Wolcott, U. S. A.,
were announced as having been elected members of the Society.

84th Meeting. March 13, 1875.

The President in the Chair.

Forty members and visitors present.

Dr. A. Woeikoff, of Russia, by request of the President, communicated

THE RESULTS OF A RECENT DETERMINATION OF THE ELEVATION OF THE CASPIAN AND ARAL SEAS,

by Col. Tillo, of the Russian Staff Corps.

(ABSTRACT.)

The levelling between the Caspian and Aral Seas was made in 1874 under the direction of Col. Tillo. It gave 243 feet difference between the two. The Caspian is considered to be 89 feet below the level of the Black Sea; thus the Aral Lake is 154 feet above the Black Sea. It was also proposed by the former secretary of the Russian Geographical Society, Wenioukef, to run a line of levels along the Bosphorus, so as to determine the elevation of the Black Sea above that of Mermara.

It is probable an expedition will start this year to level the

country between the Ural Mountains and Lake Baikal. It will be extremely important in giving us a correct knowledge of the elevation of a great part of the Asiatic Continent. Barometric observations are entirely inadequate for this, as we do not know the normal pressure which must prevail there.

The expedition is again to be conducted by Col. Tillo. Besides the help afforded by the Geographical Society, there are 7000 roubles of private contributions to the expenses of this work, and more money is expected.

Dr. Woeikoff also gave an account of

METEOROLOGICAL OBSERVATIONS IN PERU, AND OF SOME OF THE METEOROLOGICAL CONDITIONS IN THAT COUNTRY,

and stated, in reply to inquiries, that Lake Titicaca is more than 12,000 feet above the sea-level; that the changes there of mean diurnal temperature were very small, but there was often a difference of 40° between the day and night temperatures; and therefore its vicinity was not suitable for an astronomical observatory.

Mr. Gill remarked that in Lakes Bacal and Titicaca are several species of fish of peculiar types not found elsewhere.

Mr. Joseph Henry made a communication on

THE GLACIAL THEORY,

remarking that in all theories on the subject, attempts were made to explain the abnormal cold; but the enormous accumulation of snow also required explanation. He presented an hypothesis, which he had adopted many years ago, of extensive outbursts of submarine volcanoes in the equatorial regions, sending out immense volumes of steam, which, carried to a high elevation and flowing northward, would be precipitated as snow of an abnormally low temperature. More of this snow falling in winter than was melted in summer, the accumulation in many successive years would be sufficient to satisfy the demands of the glacial theory; while the power sufficient to move boulders would be produced by the changes in this accumulation from cracks and fissures, and their filling up with water subsequently freezing.

The earth in that period, as a whole, may have had a higher temperature than that which it has at the present time. This theory rests on a single hypothesis—that of the existence at the

time of submarine volcanoes which supplied the vapor; the cold being due to that of the higher atmosphere brought down by the snow from that region.

He also remarked on the great irregularities, which were probable. Catastrophes may have occurred when there were sudden eruptions of vapor, and great geological changes.

Mr. HILGARD referred to the rapid accumulation necessary to account for animals overtaken and buried in the snow. The accumulation would be more or less rapid, and the time required less or greater, as the precipitation in winter exceeded the evaporation in summer.

Mr. BESSELS referred to the very slow changes in the glaciers of the Arctic regions at the present time.

Mr. ENDLICH spoke of the impossibility of determining whether the glacial formations were rapid or slow; and of the very long time required for geological changes.

Reference having been made to the elephants and mammoths found embedded in frozen earth in Siberia, Mr. GILL remarked that it was not necessary to suppose a warm climate as necessary to their existence, as there were indications that these animals had become adapted to a quite cold climate; and instanced lions and tigers, usually regarded as tropical animals, as found at the present day in the neighborhood of the Amoor River, and adapted by a vigorous growth of hair to live in cold regions.

He further remarked that from animal remains it would seem that the preglacial period was warm, and that there followed a diminution of temperature until a minimum was reached in the glacial period. In the miocene period there was warm water and a warm temperature in the north; and there were, doubtless, gradations while these mammoths existed; and finally it became too cold for their existence, and they died out. He concluded that the transition from the miocene to the glacial period may have been gradual.

Mr. DALL remarked that the mammoths in Alaska must have lived through the glacial period. The ice-cliffs in that region must have been formed in a period of intense cold. Glaciation was not the same in different regions of the north.

He referred, also, to the source of ivory on the northern coast of Siberia, remarking that the animals may have been caught in

ice, but that their being found frozen up at the mouth of a river did not necessarily indicate glaciation.

Mr. MEIGS called attention to the fact that the tusks were found under water, and that the animals may have floated down in water, instead of having been carried by ice.

Mr. HENRY also made a second communication

ON FOG SIGNALS AND ABNORMAL CONDITIONS OF SOUND,

in which he spoke of the great attention which the Light House Board of the United States had given to fog signals, and mentioned the steam-whistle, the trumpet, and the syren, and an instrument for estimating the intensity and penetrating power of sound, and the experiments which had been made with them; in reference to recent instances of several cases of abnormal phenomena of sound.

(*This paper forms part of the Reports of the U. S. Light House Board.*)

85TH MEETING. MARCH 27, 1875.

The President in the Chair.

Thirty-two members and visitors present.

Mr. E. B. ELLIOTT read a paper on

CALENDAR FORMULÆ,

explaining more particularly and illustrating, by examples, the following expressions:—

$k + m + d - 7n = w$, in which

$k = y + \frac{y}{4} + 4 - c$, for the years of *old style*,

$k = y + \frac{y}{4} + 6 - 2r$ " " *new style*.

In these, k, the year number, is, in general, after rejecting the sevens, the number of the day of the week for a common year of the preceding December 30, for a leap year of the preceding December 31, but for the first two months of a leap year must be diminished by 1; m, the month number, is the day of the week on which the first day of the month falls in a common year, which opens on the first day of the week, and is unchangeable; *i. e.*, the number will be for

January	1	May	2	September	6
February	4	June	5	October	1
March	4	July	0 or 7	November	4
April	0 or 7	August	3	December	6.

d is the day of the month under consideration; $7n$, the largest multiple of 7 contained in $k + m + d$; c, the hundreds of the year; y, the excess above the hundreds; and r, the remainder after dividing c by 4.

The general formula may be read thus:—

To the year number add the month number and the day of the month, and the excess over the largest contained multiple of 7 is the number of the day of the week.

For years B. C., *subtract the given year less one from* 2800, *or some multiple of* 2800, *and use the above expression of k for either old or new style.*

Mr. Frisby and Mr. Gill referred to instances where dates were given both in the *old* and *new styles*, the latter stating that this was usually done in the publications of Russian scientific societies.

Mr. Schott called attention to the difference of a day in American and Russian dates in Alaska, the latter being in advance, the American Sunday corresponding to the Russian Monday.

Mr. W. B. Taylor gave an account of

A CALENDAR PROPOSED BY A PERSIAN ASTRONOMER IN 1079.

(ABSTRACT.)

There is one calendar scheme not included, I believe, in Mr. Elliot's formulæ, which appears to me of sufficient interest to deserve a notice. More than five hundred years before the adoption of the calendar of Gregory, a Persian astronomer, Omar Cheyam, one of a commission appointed by the Sultan of Khorassan to reform the calendar, proposed (A. D. 1079) a very simple modification of the Roman or Julian system, by postponing for one year, every *eighth* Julian leap year, giving the continuous succession of seven quadrennial periods, followed by one quinquennial period. That is to say, the leap year which by the Julian calendar should take place on every 32d year was uniformly carried forward so as to fall on every 33d year. This delay by one year would in four such periods, or in 132 years,

produce a retardation of leap year by one complete Julian cycle, equivalent to a suppression of one intercalary day every 132 years; in which period there would be by the calendar of Julius Cæsar 33 leap years, and by the calendar of Omar Cheyam only 32 leap years. I am under the impression that this calendar was actually adopted by the Persian Sultan, but cannot speak with confidence about it.

The Julian year of 365 days and 6 hours gains over the tropical or equinoctial year of 365 days, 5 hours, 48 minutes, and 49 seconds, an excess of one day in about 129 years, and the beautiful simplicity of Omar Cheyam's plan depends on the circumstance of the number 128 being happily divisible twice by 4. And thus, while the error of our present calendar (the Gregorian), with its inconveniently long cycle of 400 years, runs out into an excess of one day in 3756 years, the error of Omar's calendar, with its short cycle of 33 years, amounts to an excess of one day only in 5252 years. That is, it has a clear range of 52 centuries against the 37 centuries of our Christian calendar. And by reason of the shortness of the cycle, its evagation or intermediate range of departure from the true equinoctial points is also, of course, very much less.

On the whole, I must confess a great admiration for this extremely simple adjustment, and a decided preference of it to the system in common use, even when made still more accurate by the suggestion of Delambre, that one of the Gregorian leap year days should be omitted on every millennium divisible by 4000. Even this third approximation, with its cumbrous cycle, would still leave an error of a deficiency of one day in 216,000 years.

The only consideration that occurs to me as likely to be suggested in favor of the adopted calendar, is its mnemonic guide to the leap year by means of the familiar divisor 4. This is a point which, of course, would have a very different value with different minds. To the Jew or the Mohammedan, employing a different chronological epoch, no such advantage is presented. If this mnemonic have, however, any real importance, it could be equally well secured by simply omitting one leap year every 128 years. Nor would this period be practically any more arbitrary than the 400 year cycle; as is well illustrated by the general popular confusion which took place in the year 1800, when very few persons were able to say whether it was a leap year or not. The error of this scheme would be a deficiency of one day in 18,776 years.

The President communicated two letters from Mr. A. C. Ross, of Zanesville, Ohio,

ON LATENT IMPRESSIONS ON POLISHED GLASS PLATES PRODUCED BY HEATING THE PLATES IN CLOSE CONNECTION WITH ENGRAVED METALLIC PLATES;

the impression becoming visible when breathed upon. Silver appears to produce the best impressions, nickel the next, copper the least. No other metals appear to have been tried. The impressions were generally positive, but a few cases of negative impressions are described.

Mr. HENRY referred to similar experiments made by Mosher many years ago.

Mr. HENRY made a communication

ON ELECTRICITY ENGENDERED BY THE DRIVING BELT OF THE MACHINERY FOR VENTILATING THE CAPITOL AT WASHINGTON,

referring particularly to leather belts, and ascribing the electricity to compression and tension, instead of friction. Large quantity of electricity, with small intensity, is produced. Protection against fire from it in cotton factories was effected by interposing screens of glass. At the Capitol, in Washington, the electricity was utilized for medical purposes by arranging points and conductors, so as to collect it, and had been effectively applied in cases of nervous diseases. He found the electricity produced by the belt was negative.

He also made remarks on the method of lighting gas burners, by what was by some considered the electricity of the body, but which was simply the result of the friction of the shoes on the carpet. The phenomenon had become more common since the introduction of heated air from furnaces into our dwellings.

Mr. PARKER referred to several cases where sufferers had obtained great relief from the application of electricity at the Capitol.

86TH MEETING. APRIL 10, 1875.

The President in the Chair.

Forty-eight members and visitors present.

The President announced the election of Dr. ROBERT FLETCHER, Lieut. FRANCIS V. GREENE, U. S. Eng'rs, Prof. ALMA H. THOMPSON, and Hon. SAMUEL SHELLABARGER as members of the Society.

Mr. J. E. Hilgard gave a detailed

ACCOUNT OF PROGRESS OF THE INTERNATIONAL METRICAL COMMISSION,

of which he is a member.

Mr. J. J. Woodward read a brief

EXPLANATORY NOTE IN REGARD TO THE DIAGNOSIS OF BLOOD-STAINS,

by Dr. Jos. G. Richardson, of Philadelphia, from the *American Journal of the Medical Sciences* for April, 1875, p. 575, and said that he had requested permission to read this note, because in his paper "On the similarity between the red blood corpuscles of man and those of certain other mammals, especially the dog," etc., read before the Society a few months since, he had criticized a previous paper by Dr. Richardson (see *American Journal of the Medical Sciences*, July, 1874, p. 102), and that gentleman was very anxious that his defence should also be heard by the Society.

It would be noticed that in the explanatory note just read, the facts on which Dr. Woodward's criticism was based were substantially conceded, and Dr. Richardson's defence hinges entirely on the notion that it was his duty not to state these facts. This question had been so fully discussed in Dr. Woodward's former paper, however, that he did not think it necessary to say anything further on the subject at present, except that his views as then expressed are not modified by Dr. Richardson's note.

Mr. S. Newcomb, in a communication

ON THE TRANSIT OF VENUS,

gave a summary of the results obtained by the different parties, so far as the reports have come to hand.

A memoir of Mr. Robert Ridgway, entitled

OUTLINES OF A NATURAL ARRANGEMENT OF THE FALCONIDÆ,

was read and commented upon by Mr. Gill

(*This has been published in the Bulletin of the U. S. Geographical and Geological Survey of the Territories.* Second Series.)

87TH MEETING. APRIL 24, 1875.

The President in the Chair.

Thirty-eight members and visitors present.

Mr. E. B. ELLIOTT made a communication

ON AFFECTED QUANTITIES OF THE FIRST ORDER.

Mr. A. R. SPOFFORD read a paper

ON PROPOSED REFORMS IN SPELLING THE ENGLISH LANGUAGE.

Dr. ASA GRAY, of Cambridge, Mass., made remarks on the genus Torreya, discovered by Mr. CROW near the Chatahoochee, but found more abundantly near Cedar Bluff, on the Appalachicola River, and only in these localities. A species of Croonia is associated with it. Another Torreya is found in Japan, and another Croonia is associated with it.

He also referred to several genera common and peculiar to the eastern part of America and to Japan, some of which are also found in California.

Mr. GILL mentioned several species of fishes and mollusks common to America, Japan, and China, and referred to the difference between the fauna of the Pacific slope of the Rocky Mountains and that of the eastern part of North America.

Remarks were also made by Mr. E. B. ELLIOTT, Mr. DUTTON, and Mr. ALVORD.

88TH MEETING. MAY 8, 1875.

The President in the Chair.

Thirty-seven members and visitors present.

Mr. J. E. HILGARD gave an account of experiments on

IRON FACING COPPER PLATES,

at the office of the U. S. Coast Survey.

Two important results were obtained from these experiments; the face of the plate was rendered very much harder than the copper-plate, permitting ten times the number of impressions to be taken from it; and second, the plate was rendered, by the "iron-facing" process, permanently magnetic, having a polarity north and south in the vertical direction of the plate when the deposit was made.

Remarks were made by Messrs. DUTTON and E. B. ELLIOTT, and by Mr. HENRY on the magnetic condition of these plates.

Mr. DUTTON made some observations on

THE CAUSES OF GLACIAL CLIMATE,

reviewing the various theories, geographical and astronomical, on the subject; rejecting, after analysis, Mr. CROLL's astronomical theory as inadequate to account for the observed facts; and prefering Sir CHARLES LYELL's geographical theory based on the distribution of land and water, as meeting the demands of the question in a more satisfactory manner.

Mr. TAYLOR, in reply to Mr. DUTTON, thought that Mr. CROLL's "astronomical" theory had not been fully presented. That theory did not assume any diminution of solar heat upon the northern hemisphere when its winter solstice occurred at aphelion during the period of maximum eccentricity, but merely a change of distribution of the same annual amount of radiation received, between a prolonged winter of increased severity, and a shortened summer of proportionally increased severity.

The great variety of physical results flowing from a slight annual accumulation of snow in one hemisphere, continuing for many thousands of years, producing an ice cap many thousands of feet thick at the pole, and extending probably half-way to the equator, displacing the centre of gravity of the earth several hundreds of feet northward, occasioning a corresponding overflow of the Northern Ocean, or an apparent submergence of considerable portions of the northern continents, affecting the relative force of the northern and southern trade-winds and a southward pressure of the thermal equator, a similar change in the ocean currents, etc., all accord well with the observed conditions of the glacial epoch, while they all serve to re-enforce and intensify the aggre-

gate effect. This astronomical fact presents at least a *vera causa*, which unquestionably must produce some secular change of climate.

On the other hand, the "geographical" theory so ably urged by Lyell is essentially speculative. We know indeed that great and repeated changes of elevation have occurred in the land, but that they have occurred in the directions required by the theory is pure assumption. There is good reason to believe that no considerable change in the positions of the continents has taken place since the commencement of the glacial epoch.

Years before Mr. Croll advanced his theory, there was a growing opinion among several leading geologists, such as Cumming, Goodwin, Austen, Ramsay, Page, and others, that we had indications of colder periods of long continuance, in the Mesosoic and Palæozoic ages; notably in the Cretaceous, and in the Permian, the Devonian, and even in the remote Cambrian formations. Such recurrences of what may be called glacial epochs, would of themselves seem to point rather to cosmical, than to local or geographical causes. Any considerable change in the latitude distribution of land and water would, of course, have its effect on the general local climate; but from the above point of view, would have to be regarded rather as a perturbing, than as an originating influence.

Mr. Powell continued the discussion, objecting to Croll's theory, that it unfortunately gave fixed dates, or determinate periods, which were really as inadequate (in the view of practical geologists) for the work accomplished and the changes effected, as was the earlier Mosaic chronology. Mr. P. then gave an account of his observations on the character and extent of the erosion exhibited in our western river beds, and on the various distributions of gravels, as furnishing irresistible evidence of the requirement of far longer periods of time than had ordinarily been assigned for the formation of such conglomerates.

89TH MEETING. MAY 22, 1875.

Vice-President HILGARD in the Chair.

Thirty-eight members and visitors present.

The discussion of the

CAUSES OF THE GLACIAL PERIOD

was continued from the last meeting.

Mr. J. W. POWELL gave a description of the various gravels found in the geological formations of our northwestern valleys. These consisted of *shore gravels* on sea beds and on lake shores; *ice gravels*, deposited by floating ice and by morains; *sub-aerial gravels* formed by living streams; *desert gravels* and residuary gravels; *pre-quarternary gravels*, consisting of shore gravels, floating ice gravels, sub-aerial gravels modified, and, perhaps, morainal gravels modified. Without committing himself to either the astronomical or geographical theory concerning the origin of these, he argued that when we separate that which is due to glaciation from that which is due to other causes, the problem becomes greatly simplified, and there was not required that great change in the meteorological condition of the earth which some have supposed.

Mr. DUTTON replied to certain points made by Mr. TAYLOR at the previous meeting in favor of Mr. CROLL's theory, and argued its rejection on the ground of insufficiency. The changes in amount and distribution of solar radiation dependent upon the eccentricity of the earth's orbit seemed altogether too small for the very large conclusions that had been drawn from them. He had always supposed that the thermal equator, instead of being shifted by the eccentricity of the earth's orbit, was dependent upon the relative distribution of land and water upon the earth's surface. He passed in review several points in the astronomical theory, and concluded with saying that the attempt to fix definite limits of time within which glacial action has taken place must always be futile, as the idea of indefinite duration of time for the accomplishment of these changes was thrust upon the geologist with a force he could not resist.

Mr. B. F. CRAIG, referring to the assumption that the existence of large tracts of land near the equator had a decisive effect in increasing the general warmth of the atmosphere, said that air may rise to 120° Fah. over dry and barren land, and does not rise much over 80° on the ocean, but a cubic yard of air heated from 40° to 80° by contact with warm water takes up as much as (50) fifty heat-units (English), of which thirty-two belong to the water vaporized. When the air is again cooled to 40° the vapor is condensed, and the whole fifty units are given off.

A cubic yard heated from 40° to 120° over a dry surface takes up only thirty-four units, notwithstanding the higher temperature attained.

A cubic yard of water heated to 40° takes up (67,000) sixty-seven thousand units.

The deposition of moisture from air is a more important means in the distribution of heat than the convection by the air proper; and the supposed existence of large tracts of land about the equator will be far from conveying the effect which Mr. LYELL and others have supposed, in giving us a warmer climate. The conditions, therefore, which seemed more favorable for the existence of a tropical climate near the poles would be a large equatorial ocean, and such a formation of the continents as to draw the currents of the ocean toward one particular pole.

Mr. TAYLOR, replying to Mr. DUTTON, said that the present issue, concisely stated, appeared to be, that, while one side would impugn the *sufficiency* of the "astronomical" theory, the other disputed the *verity* of the "geographical." Mr. T. thought that one great merit of Mr. CROLL's hypothesis was that it seemed to explain a large number of varied, yet correlated, results, cumulative in their effect—from the gradual action of apparently very small differences as causes. In this it harmonizes admirably with the "uniformitarianism" which forms the basis of all our scientific investigations and theories.

In regard to the thermal equator, which is now on the average a few degrees north of the geodetic equator, it is true, as Mr. DUTTON has remarked, that it is greatly influenced by geographical arrangement. But any conditions which would change the relative force of the northeastern and the southeastern sets of

trade-winds, must also affect considerably the position of the median zone, or the thermal equator. As to the effect of land distribution on climate, this latter is perhaps even more controlled by continental configurations (which determine the deflections of ocean currents) than by merely areal distributions.

In reply to Mr. POWELL, in whose remarks on the chronology of post-tertiary erosion Mr. T. felt great interest, he would only say that he did not think that Mr. CROLL'S theory imposed any such narrow limits of time on the geologist as had been supposed. The variations of orbital eccentricity of our planet, depending on the conjoined action of the larger and nearer planets, with incommensurable periods, are, of course, very irregular, both in time and amount; and although they may be said to occur—roughly at periods of one or two hundred thousand years—yet the extreme or exceptional ranges of eccentricity (which alone may be supposed sufficient to produce any very decided results) are found to occur only at periods of one or two million years apart. Thus, if we suppose, with Mr. CROLL, that our last glacial period, or rather double period, coincided in time with the eccentricities culminating two hundred and ten and one hundred thousand years ago, the very notable maximum of eccentricity occurring eight hundred and fifty thousand years ago may represent, as suggested by Mr. CROLL, a glacial period suspected to have existed in the upper Miocene. The next preceding notable maximum occurred about two and a half million years ago.

It was erroneous to suppose, as had been intimated, that conglomerates alone had been accepted as evidencing past glacial action. Professor RAMSAY, twenty years ago, urged the glacial origin of the Permian breccias, from the large size of the fragments, from the *rarity* of rounded pebbles, from the angular and flat faced form of most of the constituents, and, lastly, from the polished and grooved surfaces not unfrequently found among them.

Mr. GILL said, that, so far as he, a biologist, had to pass on the question at issue, the 210,000 years plus the antecedent period of the Pleiocene was sufficient; it must be remembered, however, that the animal life of the earth had remained practically unchanged (except as to geographical distribution of forms) from a period long antecedent to the glacial epoch. But the fauna of

the Eocene has almost no connection with the present, or with the Pleiocene. If, then, the period specified was sufficient to account for the difference between the pre-glacial epoch and the present, a term of 850,000 years would be entirely insufficient for the period that must have elapsed since the commencement of the Tertiary.

90TH MEETING. JUNE 5, 1875.

The President in the Chair.

Twenty-five members and visitors present.

Mr. E. M. GALLAUDET read a paper on

UNCONSCIOUS CEREBRATION,

as evinced by mnemonic action; citing various instances of persons, scenes, and events suddenly recalled to the mind by perceptions of sight, hearing, or touch, without any exercise of the will.

Remarks were made by Messrs. HILGARD, HENRY, TAYLOR, and others.

Mr. O. T. MASON exhibited

ARCHÆOLOGICAL SPECIMENS,

stone implements of an early age; describing them and others, and giving a history of their discovery in Porto-Rico.

Mr. BAIRD remarked that Mr. GEORGE LATIMER, of Porto-Rico, had made a large collection of such specimens, which he had bequeathed to the Smithsonian Institution after refusing $12,000 for them.

Mr. ASAPH HALL made a communication on

APPROXIMATE QUADRATURES;

describing the various methods which had been employed in determining approximately an area comprised between a curve,

two ordinates, and the intercepted axis of abscissæ, showing that by the methods of GAUSS, in which the ordinates are taken at unequal but selected distances, greater accuracy could be obtained with fewer ordinates, than where the ordinates are taken at equal distances. In the case of a semicircle the degree of approximation was twice as great with three ordinates specially selected, as with five equidistant ordinates. The barometric curve could be better determined from three observations made at $\frac{1}{9}$, $\frac{1}{2}$, and $\frac{8}{9}$ of the interval from sunrise to sunset, than from five observations at equal intervals during the day.

Mr. B. ALVORD made remarks on

THE MORTALITY AMONG ARMY OFFICERS

during fifty years, from 1824 to 1873, as derived from Army Registers.

(*This paper is published in the Proceedings of the American Association for the Advancement of Science, at its session in August*, 1874)

He also described a proposed plan for government providing for the life insurance of persons connected with the army.

Remarks were made by Mr. E. B. ELLIOTT on the desirableness of extending such a plan to the civil service; and by Mr. TAYLOR on the policy of government providing for all kinds of insurance.

91ST MEETING. JUNE 12, 1875.

The President in the Chair.

Twenty-eight members and visitors present.

Mr. J. R. EASTMAN gave an abstract of a paper on

THE COMPARISON OF RAIN-GAUGES AT DIFFERENT ELEVATIONS;

describing observations made at the Naval Observatory in Washington. Rain-gauges less than 2 inches in diameter gave unsatisfactory results. During a year those at an elevation of 84 feet from the ground gave 88 per cent. of the amount of rain

received at an elevation of only 2 feet. The greatest difference was observed in continued slow rain, when the air was saturated with moisture; the least difference in sudden showers of short duration.

Remarks were made by Mr. HENRY and Mr. HILGARD, the former referring to observations made in one of the towers of the Smithsonian Institution.

Mr. E. B. ELLIOTT read a paper on

THE MUTUAL RELATIONS AS TO PRICE OF GOLD, SILVER BULLION, SILVER COIN, AND GREENBACKS.

Mr. O. T. MASON read a paper on

THE CLASSIFICATIONS OF OBJECTS OF ARCHÆOLOGY.

Mr. GILL called attention to the distinction between archæology and ethnology, and suggested some changes in Mr. MASON'S classification.

Mr. J. E. HILGARD gave an account of

THE MEASUREMENT OF A BASE-LINE FOR THE PRIMARY TRIANGULATION OF THE UNITED STATES COAST SURVEY NEAR ATLANTA, GEORGIA.

Three separate measurements of this line were made; the first two at low temperatures, the measurements being made in opposite directions, in order to test the effect of slopes upon the performance of the apparatus. The third was made in summer at a high temperature, in order to test the accuracy of the correction for temperature. The essential features of the apparatus were described, and the mode in which compensation of the two measuring bars for temperature is effected—the system being to compare these compound measuring bars with a simple standard bar of iron of six metres in length at the time of measuring the base, and as nearly as practicable at the average temperature which obtained during the measurement. The mechanical compensation of the bars is thus only required to maintain a nearly uniform length during the variations of temperature in the measurement, and the actual length at the mean temperature of measurement is taken account of by the known dilatation of the

standard bar. Mr. HILGARD described the experiments which had been made for the determination of this dilatation, and exhibited a diagram showing the great accordance of the results that had been obtained for determining this essential element. The base-line in question was nearly six miles in length; and permanent monuments having been placed at intervals of about one mile, the three measurements give eighteen comparisons of measure, which show in every instance a very close agreement. The following are the differences of the several measurements of the entire base-line of 9339 meters from their mean, and the mean temperatures at which they were obtained:—

Diff. of length . .	—8.1	—0.3	+8.4 mm.
Temperatures . .	51°.4	44°.7	90°.7 Fahr.

Showing a maximum divergence of the results of about one-millionth part of the whole length. Mr. HILGARD claimed, in conclusion, that no base-line of such length had before been measured with the same degree of precision and with such abundant evidence of the same. A full account of this operation will be published in the Coast Survey Report for 1873.

92D MEETING. OCTOBER 9, 1875.

Vice-President TAYLOR in the Chair.

Twenty-four members and visitors present.

Mr. E. S. HOLDEN exhibited and made remarks on

TWO DRAWINGS OF NEBULÆ, MADE WITH THE XXVI.-INCH TELESCOPE OF THE U. S. NAVAL OBSERVATORY, BY MR. L. TROUVELOT, OF CAMBRIDGE, MASS.

(ABSTRACT.)

Mr. TROUVELOT was invited by the Superintendent of the U. S. Naval Observatory to visit the Naval Observatory, and to use the xxvi. inch equatorial for the purpose of making drawings of planets and nebulæ. Mr. TROUVELOT was enabled to remain only from Sept. 21 to Oct. 1, 1875, and hence less time was bestowed than would be required for a perfectly satisfactory representation of any of the objects viewed. Nevertheless, good pastel draw-

ings on a large scale were obtained of, 1st, the planet *Saturn;* 2d, of the central portion of the nebula of *Orion;* and 3d, of the Horse-Shoe Nebula (G. C. 4403). The scale of the 1st is about 12 inches to 18″; of the 2d, 1 inch to 32″; of the 3d, 1 inch to 129″.3. Of these, the 2d and 3d were exhibited to the Society. The drawings contain only what was seen certainly, there being no conjectures recorded; and that of the Horse-Shoe Nebula is almost entirely satisfactory. The nebula of *Orion,* however, must be regarded as a preliminary sketch, as this object is much too complicated for satisfactory representation in the limited time at the disposal of Mr. TROUVELOT. It will be corrected during the next opposition.

The method pursued in making the drawing of the nebula (G. C. 4403) consisted in putting in the stars in the neighborhood of the nebula by means of a glass reticle ruled into squares. The sides of the squares were about 1′ of arc. The entire number of stars was put in in one night's work. Mr. HOLDEN has determined the average error of a star-position, in this nebula, to be about 7″.7 in R. A., and 5″.3 in Declination, by comparison of 11 of Mr. TROUVELOT'S star-positions with 11 of LASSELL'S (Mem. R. A. S., vol. xxxvi. p. 49). The larger residual in R. A. is accounted for by the fact that the driving-clock of the equatorial was not performing well. This average error of a star-position would not be appreciable in any drawing of the nebula which could be put on an ordinary *quarto* page. The stars of the *Nebula Orionis* were put in from G. P. BOND'S catalogue.

Mr. HOLDEN verified most of the details of the drawings—especially in the brighter parts of the Horse-Shoe Nebula, where variation of form may be suspected—and was able to declare the pastel drawing to be very nearly correct, not only in general effect, but as to the important details. A decided difference exists between some of the previous drawings and this one in the neighborhood of Lassell's star No. 1. At present, the brightest nebulosity *follows* this star. LASSELL, MASON, and others have drawn it *preceding.* The great merit of pastel drawings for this kind of delineation is that changes can easily be made, and that the *effect* of nebulous matter can be successfully reproduced.

Remarks were made by Messrs. COFFIN and TAYLOR.

Mr. E. B. ELLIOTT read the following paper on

MUTUAL RELATION AS TO PRICE OF GOLD, GREENBACKS, SILVER BULLION, AND SILVER COIN.

Gold Price of Silver Bullion.—Owing to the large demand for gold, and the corresponding diminution in the demand for

silver, consequent upon the change by certain continental governments—Germany and the Scandinavian Governments of Sweden, Norway, and Denmark—from a silver standard for their money of account, to a gold standard; and to the hoarding of gold by the Bank of France preparatory to its resumption of specie payments; and, also, to the large production of silver from the Comstock and other mines of our silver-bearing territories, the price of silver, relatively to gold, has been for several years gradually sinking, until it has reached in the London market, according to a late cable dispatch, the low point of 55½ pence sterling per standard British ounce (the equivalent quotation in New York market, for fine bars, being from 1.21 to 1.22 per ounce), the lowest point of value, relatively to gold, on record in the history of man.

The price indicates that the ratio of the value of gold to silver, is as 17 to 1; that is, that the value of gold in the markets of the world is now seventeen times that of silver of equal weight and like fineness.[1] It follows, as may readily be shown, that the price in United States gold of the quantity of bullion contained in a dollar of our new fraction silver is 88.0 cents; or, conversely, that what may be termed the silver-bullion price of gold—the silver unit being 25 grammes nine-tenths fine—is 113.6.

Gold Price of Silver Coin.—The gold price of the United States silver coin (fractional), which is used as currency—containing to the dollar, when of legal weight and fineness, 25 grammes nine-tenths fine—was quoted on the same day (June 5, 1875), in the New York market, at from 92 to 95 cents, showing the silver coin price of gold to have been from 105.3 to 108.7; the difference depending on the quantity of pure metal contained, as indicated by the date of the mintage and by the degree of the abrasion of the coin.

Gold Price of Greenbacks.—The greenback price of gold is now quoted, in the language of the market, at 117, showing the gold price of greenbacks to be 85½.

Greenback Price of Silver Bullion and of Silver Coin.—It

[1] June 6, 1876. According to the latest quotations in the London market the price of silver has fallen to 52 pence per ounce of the British standard of fineness (to wit, $\frac{111}{120}$ fine), indicating that the ratio of the value of gold to silver is now as 18 to 1.

follows that, at the present time, the greenback price of silver bullion—25 grammes nine-tenths fine to the dollar—is 103 (more exactly, 102.9); the greenback price of silver coin, of the same weight and fineness, ranging from 107.6 for older and abraded coins, to 111.1 for coins of the full legal weight.

Exportation of Silver Coin for Melting or Recoinage.—Should the price of greenbacks, relatively to that of silver bullion, advance three per cent., silver coin, even at its minimum or bullion value, would prove more profitable for circulation as money than for use in the arts, or for exportation for coinage abroad.

Exportation of Silver Coin for Use as Money Elsewhere.—The fractional silver coin of the United States is demanded, in limited quantities, by certain South American and other countries, chiefly on this side of the Atlantic, for circulation; which fact accounts in part for the higher price which our fractional silver coin commands in the market compared with bullion. To what extent this fact will operate—when a liberal supply of coin shall be issued and thrown upon the market—to retain as now, the price of coin beyond that of bullion, is, as yet, uncertain.

Effect on the Price of Silver Coin of the Demand for its Use at Home as Subsidiary Coinage.—Gold and greenbacks are each legal tender of payment in all amounts, but United States fractional silver coin is legal tender of payment only in limited amounts, not exceeding five dollars in any one payment. The effect of this provision of law is to give to silver coin a value superior to its intrinsic value as bullion, and to protect it against remelting at home, and against exportation for melting or recoinage abroad.

When greenbacks rule in the market at a lower point than that of silver coin, such coin will not be in demand at home for use as money for general circulation, except at the extreme Southwest and on the Pacific Slope. When, however, greenbacks command in the market a higher price than silver coin, the subsidiary silver coin will be in demand as money, but will command a price above that of its value as bullion.

When greenbacks advance from 85½, their existing rate, to 88, the existing bullion rate (corresponding to a premium on gold of 13.6)—assuming that the relative values of gold and silver remain unchanged—silver coin will necessarily cease to be profitably

exported as bullion for melting or recoinage, even though—by virtue of the provision of the law which gives it the character of a legal tender in limited amounts—it should have no value above that of the bullion contained.

When greenbacks advance to 92 (corresponding to a premium on gold of 8.7)—the existing prices of silver coin remaining unchanged—the less perfect and less desirable silver coins will circulate, as currency, side by side with the fractional paper currency.

When greenbacks advance to 95 cents (correspoding to a premium on gold of 5.3)—the existing prices of silver coin remaining unchanged—the new and more perfect silver coins will circulate, as currency, side by side with our fractional paper currency.

When greenbacks advance beyond this rate, nearer to a par with gold, silver coin will supersede greenbacks, and their associated fractional paper currency. The intrinsic bullion value of new silver coins, of legal weight and fineness, is now 88 cents to the dollar, but their value as coin in the market is 95 cents. The issuing and placing on the market of the new silver coinage in considerable quantities, will tend to lower somewhat the price in the market of these new coins, but will not reduce the price to the bullion standard. The gold price of greenbacks in the market, therefore, must advance considerably beyond 88 cents, the present value of the bullion contained in silver coins of legal weight and fineness—that is—the greenback price of gold must fall considerably below 113.6, in order to secure the free and general circulation of such coins.

The higher price of our fractional silver coin, as compared with silver bullion, of the same weight and fineness, is due, in part, to the fact that, in limited amounts (not exceeding $5 in any one payment), it is, like gold, a legal tender of payment in the United States, and, in part, to a limited demand for its use as money in the payment of balances for custom purposes, and for the settlement of fractional amounts in the payment of interest on our bonded debt; also, for use in general circulation on the Pacific slope of the United States and in Texas and certain other portions of the southwest, where gold is the sole standard, paper currency not being recognized in trade; and, also, in certain South American and other countries.

The Trade Dollar—its value in Gold and in Greenbacks.—A word as to the trade dollar. The trade dollar contains of silver, of the usual standard of fineness of nine-tenths, 420 grains troy; its weight is, therefore, 16.2791 times that of the gold dollar.

Its value as bullion when gold is worth seventeen times silver of the same weight, is 95.78 cents. Its price in the market, mainly owing to its demand for Oriental circulation, is from 97 to 98 cents gold, or about 2 cents in advance of its value as bullion.

TABULAR STATEMENT.

In the following tabular statement, the dollar of silver bullion, and the dollar of silver coin, are each assumed to be 25 grammes of silver of the fineness of nine-tenths—the same with regard to quantity and fineness, as that of the legal silver currency (fractional) of the United States:—

PRICES—*June* 5, 1875.

The gold price of $100 in greenbacks, is		$85.50
The gold price of $100 in silver bullion, is		88.01
The gold price of $100 in silver coin, is from		92 to 95
Consequently—		
The greenback price of $100 in gold, is		117.0
The silver bullion price of $100 in gold, is		113.6
The silver coin price of $100 in gold, is	from	108.7
	to	105.3
Also—		
The greenback price of $100 in silver bullion, is		102.9
Greenback price of $100 in silver coin,	from	107.6
	to	111.1
Also—		
The silver bullion price of $100 in silver coin, is	from	104.5
	to	108.0
The silver bullion price of $100 in greenbacks, is		97.2

Mr. T. N. GILL read a paper on

THE PROGRESS OF THE NATURAL SCIENCES DURING THE PAST CENTURY.

(*This is published at length in Harper's Monthly Magazine, February*, 1876.)

93D MEETING. OCTOBER 23, 1875.

The President in the Chair.

Thirty-two members and visitors present.

Mr. JOSEPH HENRY made further remarks on

SOUND IN CONNECTION WITH FOG SIGNALS,

referring particularly to echo, or reflection of sound, as observed in experiments under the U. S. Light-House Board.

(*These experiments are described in the Report of the Light-House Board for* 1875.)

Mr. W. B. TAYLOR made a communication on

ACOUSTIC REFRACTION.

(ABSTRACT.)

Sound, though differing from light in the character of its waves and their order of magnitude, yet moves like light in radial lines, and, like light, is diverted from its rectilinear course whenever its waves undergo an unequal retardation or acceleration.

There are three different methods in which sound-waves passing through a gaseous medium may suffer such unequal disturbance of velocity: First, by variations of *density* in the medium—sound moving more slowly as the square root of the density, the pressure being the same. Second, by variations of *elasticity* in the medium, sound moving more swiftly as the square root of the elasticity, the density being the same. Third, by variations of motion or *current* in the medium—sound travelling (by convection) faster with the wind by a small percentage according to its velocity—and conversely.

There is no doubt that light also is liable to all three of these forms of refraction; as its velocity is necessarily retarded by an increase of density in the medium, by a reduction of its elasticity, and by an adverse motion in the medium.

A fourth cause of velocity disturbance in the case of sound exists in the *temperature* of the medium—sound moving more swiftly in a heated atmosphere, in proportion to the square root of the absolute temperature. As the only dynamic effect of heat on a gas is to increase its elasticity by confinement, if the volume be constant, or to increase its volume if unconfined, this cause of acoustic refraction, important as it is practically, may be theoretically resolved into one of the preceding conditions.

1. The refraction of sound resulting from differences of *density*

was first exhibited by SONDHAUSS, in 1852, by means of a lens of carbonic acid gas confined in an envelope of gold-beater's skin, or preferably, of collodion film. The wave-fronts of sound (considered as practically plane surfaces) being centrally retarded in passing the convex surface of the lens, thus move through and emerge from the lens with concave surfaces, whose normals converge to a point. A convex lens of hydrogen would cause the wave-fronts of sound passing through it to emerge (by acceleration) with a convex form—that is, would cause the rays or normals to diverge—the focus being negative. And to obtain a positive focus of convergence it would be necessary to make a hydrogen lens concave. The same effect would be produced on light by a lens of hydrogen.

2. The refraction of sound resulting from differences of *motion* in the air was first suggested by Prof. STOKES, in 1857. As the advance of sound is always in directions normal to the expanding spheroidal surfaces of instantaneous compression, any deformation of these spheroidal surfaces must correspondingly deflect the line of successive impacts from the original radial direction. Winds being usually considerably retarded near the surface of the earth by frictional resistance, the wave-front of sound moving in the direction of the wind, is more advanced above than below, causing the sensible rays to bend downward; on the other hand, the wave-front is more retarded above than below when opposed to the wind, causing the rays to bend upward. Prof. HENRY was the first to observe (in 1865) that sound moving against the wind could be heard aloft after it had ceased to be audible below, though it was not till some time afterward that he detected the true cause. Prof. REYNOLDS subsequently (in 1874) independently verified by experiment the theory of Prof. STOKES.

3. The refraction of sound, resulting from differences of *temperature*, was first pointed out by Prof. REYNOLDS in 1874, who has shown that various recorded observations on sound very strikingly establish the indications of theory in this direction.

These last two conditions of acoustic refraction—inequality of motion in the air and inequality of its temperature—are both susceptible of very simple quantitative determination, and are thus shown to be real and efficient causes of many observed results, and to furnish satisfactory explanations of many hitherto puzzling phenomena of sound. Various illustrations were given.

Mr. DUTTON spoke of the rumbling sound of trains on the Alexandria Railroad, as heard at the U. S. Arsenal in this city; sometimes loud, sometimes moderate, sometimes inaudible; much louder with favorable than with adverse winds; sometimes, especially if the air was still, the sound, while inaudible at the ground, was distinct, or even loud, at an elevation of 40 feet.

He also suggested that diffusion might often compensate for refraction of sound.

Further remarks were made by Mr. TAYLOR and Mr. HENRY.

94TH MEETING. FIFTH ANNUAL MEETING, NOVEMBER 6, 1875.

Vice-President TAYLOR in the Chair.

Twenty-four members present.

The following officers of the Society were elected for the ensuing year:—

President,	JOSEPH HENRY.
Vice-Presidents,	J. K. BARNES, W. B. TAYLOR,
	J. E. HILGARD, J. C. WELLING.
Treasurer,	PETER PARKER.
Secretaries,	J. H. C. COFFIN, T. N. GILL.

MEMBERS OF THE GENERAL COMMITTEE.

CLEVELAND ABBE,	N. S. LINCOLN,
S. F. BAIRD,	S. NEWCOMB,
C. E. DUTTON,	O. M. POE,
E. B. ELLIOTT,	C. A. SCHOTT,

J. J. WOODWARD.

95TH MEETING. NOVEMBER, 20, 1875.

The President in the Chair.

Thirty-nine members and visitors present

Mr. E. B. ELLIOTT made a communication on

ADJUSTMENT OF THE CALENDAR:

describing several propositions and attempts to remedy its defects, and the advantages of alternate months of 30 and 31 days, or

sextiles of 61 days, in leap years; the last month, or sextile, in common years to have one day less. He explained and illustrated by examples the transition from the present system.

Mr. COFFIN suggested that this transition would be simplified by beginning the year one day earlier than at present.

Mr. J. J. WOODWARD made a communication on

DIFFRACTION PHENOMENA IN THE FIELD OF THE MICROSCOPE:

illustrating by photographs of *Frustulia Saxonica* thrown upon a screen, and showing that spurious striæ may be produced by throwing the light obliquely on the object.

(*The paper is published in the Monthly Microscopical Journal, December,* 1875, *p.* 274.)

The President read, as his Annual Address,

AN ACCOUNT OF RESEARCHES ON SOUND IN ITS APPLICATION TO FOG-SIGNALS.

(*This forms part of the Report of the U S. Light House Board for* 1875.)

96TH MEETING. DECEMBER 4, 1875.

The President in the Chair.

Thirty-five members and visitors present.

The election of Commander E. P. LULL, U. S. N., as a member of the Society was announced.

Mr. JOSEPH HENRY read a paper on

HALF-VISION:

describing the phenomena in his own case, and illustrating by a diagram the luminous circle and colored spectra presented to his vision.

Mr. WOODWARD followed with remarks on the frequent observance of half-vision, and the amount of literature on the subject; describing an injury to his own eyes from the use of the

electric light in illuminating the microscope, the structure of the eye and optic nerve, and explaining the theory of such abnormal phenomena.

Mr. G. K. Gilbert made a communication on

RIPPLE-MARKS.

(ABSTRACT.)

Ripple-marks are observed, first, upon dry, shifting sand; second, on sand under water; third, on sandstone strata. The third case is the fossil phase of the second.

1. *As to their Form.*—The ripples on a slab of sandstone are usually equal, parallel, equidistant ridges, curved over the top, and separated by curved troughs; and the question has been raised whether the curvature of the ridges or that of the troughs is the more acute. Since the stratum which overlies a rippled sandstone may preserve on its under surface a mould, or reversed impression, of the rippling, there may be doubt in the case of a detached specimen whether it exhibits the true ripple or its mould. From an extended series of observations in Utah, the speaker concludes that the crests of the ridges are more acute than the intervening troughs, and that this rule is so little liable to exception, that it may be used in determining which is the originally upper surface of a detached or highly inclined bed of sandstone. An opposite opinion is held by Jukes (Manual, 1872, p 163).

2. *As to their Cause.*—The view is advanced that the ripples on dry and on wet sand are due to *vibrations* of air and of water, and are analogous to, if not homologous with, the accumulations of sand along the node lines of vibrating elastic plates. We know from the phenomena of rapids that running water is thrown into vibration by friction on its channel. We know from the whistling of the wind that air is given uniform vibration by friction. Are not such vibrations, arising from the friction of currents, competent, if constant in position, to produce the phenomena of ripple-marks? The following facts appear to accord with this hypothesis: First, the wavelets are, within restricted areas, sub-equal in all dimensions. Second, in the case at least of those formed under water, the wavelet does not travel, like a sand-dune, but is constant in position so long as the conditions remain unchanged. In one observed instance, the lamination of strata showed that a set of ripple-marks had held the same position while two feet of sediment were accumulated. Third, there are compound ripplings. In one fossil specimen exhibited to the Society, the ripples are double, each main ridge being supplemented by a smaller one running along its base. In another

specimen, a system of ridges is crossed at right angles by another of smaller size, a reticulation being the result.

A different explanation is given by JUKES (*loc. cit.*), and by DANA (Manual, 1874, p. 672).

3. *As to their Geological Interpretation.*—If currents are adequate to their production, they may be formed at great depth, and are demonstrative neither of shallow water nor of the proximity of shore, as they are often regarded. They are never found in shales, while few sandstone series are without them. That is, they are formed only where the motion of the water is too great to admit of the accumulation of fine sediment.

Mr. F. W. PUTNAM, of Salem, Mass., described and explained ripples formed by the tide.

Mr. POWELL described various forms of ripple-marks which he had observed, and remarked that ripple-marks may be formed in deep water from the motion of waves on the surface, and were not necessarily an indication of action on a shore.

The subject was further discussed by Messrs. HENRY, ABBE, and GILBERT.

Mr. J. J. WOODWARD made some comments on

THE MICROSCOPICAL STRUCTURE OF WOOL:

reading a report, by Dr. JOHN LECONTE and himself, to the President of the National Academy of Sciences, on the microscopical examination of many specimens of mixed goods of sheep or lamb wool, with cow or calf, and other hair, with a view to determine the proportion of the former. In illustration, he exhibited on a screen 25 photographs of various kinds of wool and hair.

The examination and report were made at the request of the Hon. Secretary of the Treasury, in order to determine the rate of duty on the several kinds of goods.

97TH MEETING. DECEMBER 18, 1875.

The President in the Chair.

Fifty members and visitors present.

The election of Mr. EDWARD GOODFELLOW, of the U. S. Coast Survey, as a member of the Society was announced.

Mr. J. A. OSBORNE exhibited and described

A NEW METEOROLOGICAL INSTRUMENT:

(ABSTRACT.)

A consideration of the effect of climate upon animal life and well-being will lead to the conclusion that the chief influence which the elements exert upon the human body is essentially thermic in its character. The tendency of the actual temperature of the atmosphere, its humidity and motion, as well as that of the direct radiant heat from the sun, is to effect a change in the normal warmth of the body.

No attempt has hitherto been made to give definite expression to the physio-thermic influence for different places on the surface of the earth, or for the same place at different times. The instrument exhibited for this purpose consists of an isolated cylinder of bank-note paper, hanging from a horizontal ring, and containing about three pounds of water raised to the temperature of the blood. A thermometer is suspended in the water, and, as the latter cools spontaneously, it is kept in continuous and perfect agitation by clock-work, so that a true determination of the temperature of the whole mass is constantly indicated. By recording in seconds the time in which the mercury sinks from degree to degree, we obtain a series, which, being reduced to a single expression, will give a number comparable with the loss of heat sustained by a human being, and proportional to other observations made with the same or a similar instrument. For it will be seen that the cylinder and its contents, having a surface which is slightly moist, is subject to the same influences which affect a man in the same locality; and its loss of temperature is determined by the combined action of radiation, evaporation, and the convection of its heat by the moving currents of air.

As an animal has to maintain a constant temperature, losing as much heat as he makes, an investigation of the external causes which determine that loss, or tend to retard it, is of preeminent importance.

Having obtained a serviceable value for the aggregate physio-thermic influence, it was proposed, by the simultaneous use of pervious and impervious cylinders, placed in and out of the wind, to analyze this total, and apportion to each of the great factors its proper share in the production of sensible heat or cold. And, finally, by an investigation of many such analyses, to establish an empirical formula by the aid of which existing meteorological records may be expressed in units of thermic value; thereby, in the interests of physiology, to extend a knowledge of the climates of the globe, subjectively considered.

Mr. HILGARD regarded it a meteorological, and not a physiological, instrument; and spoke of the value of the ordinary meteorological observations as giving much information on climates of different regions, from which their effects on man may be deduced.

Mr. WOODWARD doubted the precision of the instrument exhibited; and spoke of the practical difficulty in obtaining homogeneous porous-paper, and in constructing several instruments, which would agree in their indications.

Mr. GEORGE B. DIXWELL, of Boston, made a communication on

CYLINDER CONDENSATION, STEAM JACKETS, AND SUPERHEATED STEAM:

giving an extended abstract of a pamphlet, which he had published on these subjects.

Prof. A. M. MAYER, of Hoboken, N. J., made a communication on

A METHOD OF DETERMINING A DEFINITE INTERVAL OF TIME, AND ITS APPLICATION TO MEASURING THE NUMBER OF VIBRATIONS OF SOLID BODIES.

Mr. HILGARD and Mr. HARKNESS participated in the discussion which followed.

98TH MEETING. JANUARY 15, 1876.

The President in the Chair.

Forty-three members and visitors present.

MR. J. J. WOODWARD made

REMARKS ON THE PAPYRUS EBERS,

exhibiting and describing two quarto volumes, with colored plates, of an Egyptian medical work written 1552 years before the Christian era, and probably the oldest medical work extant. The volumes contained the hieratic text and translations into hieroglyphics and German.

Mr. PARKER and Mr. GALE followed with remarks on Egyptian antiquities.

Mr. E. B. ELLIOTT read a letter from Dr. B. A. GOULD, Director of the National Observatory at Cordoba, giving an account of

THE COINAGE OF THE ARGENTINE REPUBLIC,

as established by an act of the legislative body, which went into effect in October, 1875.

The Argentine *peso-fuerte* is of the same value as the Japanese *yen;* and 1003.08774 *pesos-fuertes* are equivalent to 1000 *dollars* of United States coin, the difference being a little more than $\frac{3}{10}$ of 1 per cent.

Mr. J. W. POWELL made a communication on

SOME TYPES OF MOUNTAIN BUILDING,

describing several characteristic forms in the Park Range, the Ute, and other mountains in the West, and pointing out marked differences between these formations and those in the Appalachian range.

99TH MEETING. JANUARY 29, 1876.

The President in the Chair.

Thirty-five members and visitors present.

Mr. W. H. DALL read a paper on

THE SUCCESSION OF THE STRATA OF THE SHELL-HEAPS OF THE ALEUTIAN ISLANDS.

(ABSTRACT.)

He showed that the shell-heaps were separated by the strata of which they are composed into deposits of three successive periods. The lower stratum being composed of the remains of echini, eaten by the early inhabitants, he termed the Echinus Layer, and the period in which it was formed the Littoral Period. The second, composed chiefly of fish-bones, the Fish-bone Layer, deposited in the Fishing Period. The upper or Mammalian Layer, corresponding to the Hunting Period, was chiefly formed

by the bones of mammals and birds. Over all these lie the much more modern village sites, very few of which are now occupied.

The first layer might have been deposited in a thousand years. There is no means of approximating to the age of the subseqnent layers. The Echinus layer contained few and very rude implements, and a gradual progression was noted in the variety and finish of the articles found in the successive layers. Only toward the last are there any signs of the use of houses, fire, or ornamental designs. The character of the implements showed that the early inhabitants used those of a pattern similar to the Eskimo, but that these gradually became differentiated into a type peculiar to the islands. Mr. DALL considered it probable that the first inhabitants were Eskimo of a low type, forced for protection from America into the islands, who in their restricted surroundings in the course of time developed into a special type without entirely effacing the traits which link them to the Eskimo, by language, physique, and fabrications.

In reply to questions, he gave reasons for his supposition that the inhabitants of the Aleutian Isles came from the East.

Mr. POWELL spoke of the *khiva* or underground apartment found in the remains of dwellings from the Arctic regions, through California and Colorado, and to the Gulf of Mexico, usually in the centre of the building, regarding it as the common workroom or assembly hall of the occupants.

Mr. MASON made remarks on the similarity of instruments and implements found in Australia, North America, and the Aleutian Isles, and the probability of there having been great changes in the bed of the ocean between North America and Asia.

Mr. EMIL BESSELS made a communication on

THE HYGROMETRICAL CONDITION OF THE AIR IN HIGH LATITUDES,

discussing observations made at Polaris Bay by the late polar expedition under Capt. HALL.

100TH MEETING. FEBRUARY 12, 1876.

The President in the Chair.

Thirty-eight members and visitors present.

Mr. G. K. GILBERT made a communication on

THE HORARY OSCILLATIONS OF THE TEMPERATURE OF THE ATMOSPHERE,

in which he correlated barometric and thermometric changes and rates of change in observations made at San Francisco and Philadelphia in the equinoctial months of March and September.

Mr. F. F. JUDD made a communication on

THE WATERSHED OF THE ADIRONDACK REGION,

giving results of a survey of the head-waters of the Hudson and Rackett Rivers by Prof. FARRAND N. BENEDICT in 1874, and dwelling particularly on the feasibility and facility of converting the numerous and extensive lakes in that region into reservoirs, so managed as to greatly reduce freshets in the Hudson in the spring, and supply the deficiency of water in the fall.

Remarks were made by Messrs. HARKNESS, HENRY, POWELL, and ALVORD.

Mr. ELISHA GRAY gave an

EXHIBITION OF A TELAPHON,

which he had invented, by which musical tones transmitted over a telegraph wire can be responded to at a distant point. Several tones of a different pitch were transmitted simultaneously by a single circuit from one room, and each responded to in another room by a reed attached to an electric magnet and tuned to the same pitch. With such apparatus several messages can be sent at the same time by the same circuit, and at the place of reception each would be distinguished from the rest by difference of tone and by being responded to by a magnet whose reed was tuned to the same pitch as that from which it had come. He stated that messages had been thus transmitted successfully by a circuit of 300 miles.

He also exhibited other methods by which the sounds could be responded to; in one of which a tune played in one room was repeated by a diaphragm in another, with which an electrical connection was made.

101ST MEETING. FEBRUARY 26, 1876.

The President in the Chair.

Forty-one members and visitors present.

The election of Mr. EDWARD J. FARQUHAR, U. S. Patent Office, Mr. M. H. DOOLITTLE, U. S. Coast Survey, and Dr. WILLIAM MCMURTRIE, Agricultural Bureau, as members of the Society, was announced.

Mr. W. HARKNESS exhibited panoramic views and gave descriptions of places visited by the U. S. Steamer Swatara, which conveyed parties to the Southern hemisphere for observing the transit of Venus in December, 1874. The places described were Bahia, Cape Town, and Hobart-town.

Mr. E. B. ELLIOTT made remarks on

TWO PROPOSITIONS, NOW BEFORE CONGRESS, FOR CHANGING THE COIN OF THE UNITED STATES,

one proposing a silver instead of a gold standard; the other to retain the present system, but reducing the standard values; neither recognizing progress towards the metric system.

Mr. W. HARKNESS made remarks on

THE METHODS OF MEASURING THE INEQUALITIES OF THE PIVOTS OF A TRANSIT INSTRUMENT,

briefly describing the methods heretofore employed, and explaining the construction and use of a SPHEROMETER CALLIPER, which he had devised for the purpose.

102D MEETING. MARCH 11, 1876.

The President in the Chair.

Fifty-two members and visitors present.

The election of Lieut. ROGERS BIRNIE, U. S. Army, as a member of the Society, was announced.

Mr. G. K. GILBERT made further remarks on

HORARY OSCILLATIONS OF THE ATMOSPHERE,

concluding that the increase of temperature from 5 A. M. to 3 P. M. was insufficient to account for the higher barometer in that period.

Mr. J. J. WOODWARD made a communication on

THE MARKINGS ON NAVICULA RHOMBOIDES,

illustrating by photographs magnified on a screen, and showing how the real markings could be distinguished from the interference lines produced by oblique illumination of the object.

Mr. W. HARKNESS continued his description of places visited while on the Southern Expedition for observing the late transit of Venus, viz., Crozet's Island and the Sandwich Islands.

103D MEETING. MARCH 25, 1876.

The President in the Chair.

Thirty-five members and visitors present.

Mr. G. K. GILBERT made a communication on

LANDSLIPS AND LAKELETS,

enumerating the various modes in which lakes originate, and describing in particular a class which occur on the slopes below certain high cliffs and depend on the manner in which the hard capping rock, usually volcanic, is undermined and parts in large blocks.

Mr. POWELL spoke of similar lakes, but in which the upper stratum was conglomerate instead of volcanic. There were lakes on the north side of the Uintah Mountains formed in the beds of old streams by dams of detritus.

Dr. DAVID MURRAY, Foreign Superintendent of Educational affairs in Japan, gave the Society an account of

THE PROGRESS WHICH HAD BEEN MADE IN EDUCATIONAL MATTERS IN THAT EMPIRE.

(ABSTRACT.)

He first described the ancient system of education which existed before the recent revolution. It was introduced from China, which in matters of learning and literature holds to Japan the position of mother country. The territorial Daimios in many instances took great pride in the maintenance of institutions of learning for the benefit of their immediate retainers. In these institutions the elements of learning, as well as the higher departments of history and political philosophy, were taught with great thoroughness. Chinese philosophy, in the works of Confucius, Mencius, and others, was taught by professors who gave their lives to the study and elucidation of these great masters.

Along with this literary and philosophical training the Japanese youth were also regularly trained in athletic and military exercises. Such institutions were maintained usually at the expense of the Daimios, and were restricted to those who were subsequently to enter the service of their masters.

Schools for the education of the mercantile, agricultural, and laboring classes were very common, but were not maintained at the government expense. Each district or neighborhood sustained its own school, and it speaks well for their appreciation of learning that under this voluntary system almost the entire population were able to read and write.

In 1872, after the consummation of the revolution which re-established the unity of the empire, a department of education was organized for the administration of all matters pertaining to schools and colleges throughout the empire. Under this department the following classes of institutions of learning have been established:—

1. *Elementary Schools.*—These are conducted in the Japanese language, and the prescribed schedule of studies for them corresponds nearly with that of American elementary schools.

2. *Normal Schools.*—To provide competent teachers for the elementary schools, government normal schools have been established, the graduates from which have been employed in the different provinces to reorganize schools and instruct the teachers in the performance of their duties.

3. *Foreign Language Schools.*—In order to raise up a class of men able to transmute foreign learning into the Japanese tongue, schools for teaching English and other foreign languages have been provided by the government. English thus becomes to Japan its learned language, as Latin was the learned language to Europe in the Middle Ages.

4. *Colleges aad Technical Schools.*—To provide for the higher departments of learning, colleges conducted in a foreign language have been organized and are now in successful operation. In this way excellent instruction may now be obtained in Law and Government, in Engineering, in Chemical Technology, in Naval

Architecture, in Medical Science, and in Naval and Military Sciences. There is also a provision made by the government to send abroad choice students who have passed with credit through the courses of study in the institution at home. They go abroad to follow up the department of science which they have already entered upon, and are expected to return and serve the government as experts in these branches, or as teachers and professors in the schools.

There is a very strong public sentiment in Japan in favor of education, and there is no indication that, in this most important part of their work in the reorganization of their government, they are likely to relax in their efforts or fail in ultimate success.

In answer to many inquiries Dr. MURRAY gave to the Society much information in regard to the operations now in progress.

Mr. HENRY remarked that Dr. MURRAY has explained the rapid progress of the Japanese in the last few years. They were not an uneducated people, though they had arrived at a stationary condition and were incapable of advance while secluded from other nations, and yet were prepared to avail themselves of the progress in education, science, and arts with which they have lately become acquainted.

Mr. O. T. MASON commenced a communication on

INTERNATIONAL SYMBOLS FOR CHARTS OF PREHISTORICAL ARCHÆOLOGY,

exhibiting charts of these symbols, and explaining them and their combinations.

104TH MEETING. APRIL 8, 1876.

The President in the Chair.

Forty-six members and visitors present.

Mr. HENRY made a communication on

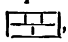
ILLUMINATING MATERIALS,

explaining the adaptation of various kinds for use in Light-houses, and the investigations and tests which had been made of their qualities.

He then introduced Mr. Mason, of Baltimore, who exhibited apparatus which he had devised for determining the explosive character of kerosene oils, and made experiments on several samples, showing the temperature at which the vapor of each would flash on the approach of a lighted match, and that at which the oil would begin to burn. The *flashing* test gave temperatures about 20° F. below the *fire* test, and the latter was several degrees below the standard of 110° F., with which the samples were marked.

Mr. O. T. Mason continued his paper upon

THE INTERNATIONAL SYMBOLS FOR CHARTS OF PREHISTORIC ARCHÆOLOGY.

He said that they sufficiently answered the characters of simplicity, distinctness, speciality, and mnemotechny. As to their universality, it was necessary for them to be not only universally legible, but also universally applicable. In this sense they were, in the first place, restricted to Europe principally, and even there failed to mark either a built-up wall, or a place of sacrifice. For the wall Mr. Mason proposed the sign , and for a sacrifice . In order to make the signs apply to America several additions would have to be made. To supply the want of a sign for an ossuary, the symbol grave is taken with a plus in the centre. A tribal lodge is indicated by the six-pointed star with a plus inclosed; an animal mound by the mound sign with the plus inclosed. Mr. Mason concluded by urging geologists of the National Surveys and others to give a portion of their leisure to the location and description of the antiquities of our country.

Mr. Powell followed with remarks on the very little knowledge that could be gathered from relics; and Mr. Gill on the information to be derived from animal remains.

105th Meeting. April 22, 1876.

The President in the Chair.

Forty-eight members and visitors present.

Mr. W. B. TAYLOR read a paper on

THE TEMPERATURE OF SPACE.

Mr. TAYLOR remarked that, although this title was derived from the illustrious FOURIER, and was in general use, it was obviously not very accurate; and that perhaps the designation "star-heat" would be less objectionable, as being the nearest analogue to star-light.

FOURIER supposed that this temperature could not be much less than the coldest degree observed in polar regions, and estimated it at —58° F. POISSON, following in his track, adopted essentially the same views and the same estimate; suggesting, however, that in the course of one million years the solar system might have passed from an external temperature of +100° C. to that of —100° C.—a demonstrably impossible occurrence. POUILLET (in 1838) estimated the "temperature of space at —224° F. HOPKINS (in 1855) placed this temperature as high as —39° F. And Sir JOHN HERSCHEL (in 1857) made an estimate somewhat lower than that of POUILLET, or —239° F.

Eminent as these names are, their results are very discordant, and are all based on assumptions of great improbability. Remembering our own equatorial "snow-line," and our tropical ice-clouds, at no greater elevation than three or four miles, it seems quite incredible that in open space, unprotected by any atmosphere, mercury could be melted (at 91,000,000 miles) even under the direct blaze of the sun. And this is about the temperature assigned by HOPKINS to star-heat alone.

Mr. TAYLOR concluded that we have really no reason for supposing that the heat radiated to us from the stars bears any higher ratio to the heat received from the sun, than star-light bears to sun-light; and that as the whole amount of light received from the stellar vault on both hemispheres does not probably exceed the ten-millionth of that received from the sun, the heat being in the same proportion, even HERSCHEL'S estimate (the lowest here cited) must be pronounced enormously too high; and the *star-heat*, commonly called the "temperature of space," cannot be much above the absolute zero.

Mr. NEWCOMB followed with some remarks, concurring in these views.

Further remarks were made by Mr. HENRY.

Mr. J. W. POWELL made a communication on

MONOCLINAL RIDGES,

of which the following is an abstract.

(ABSTRACT.)

Throughout the Rocky Mountain region there are many conspicuous topographic features found on the flanks of great ranges and elsewhere, known as monoclinal ridges. Such a ridge is composed of beds dipping in one direction. On the face of the ridge the escarped edges of the beds are exhibited. On the back of the ridge, the highest geological bed is found, and often the slope of the back conforms more or less to the dip of the beds. But where the dip is great, the higher beds are bevelled toward the summit of the ridge; where the dip is small the higher beds are cut through at the foot of the slope, so that lower beds are revealed, especially in the gulches.

In any region where the beds involved are stratified and displacement is by flexure, and upheaval is faster than atmospheric degradation, monoclinal ridges appear; such ridges being composed of harder beds, while the valley spaces are excavated in more friable material. Manifestly, all such ridges must face the axis of upheaval. In somewhat symmetric anticlinals the series of ridges appearing on one flank is also seen on the other.

Here the genesis of such ridges both in anticlinal and monoclinal flexures was explained, and the following law was stated. As upheaval progresses *pari passu* with degradation, monoclinal ridges recede from the axis of upheaval with the increase—in the amplitude of the flexure, and new ridges may appear near the axial region.

Mr. POWELL then explained the effect faults have in giving position to the ridges. He first considered the effect of a fault at right angles to the axis of upheaval, and hence at right angles to the monoclinal ridge. In such a case, upheaval is arrested on one side of the fault, but continues on the other, both in anticlinal and monoclinal flexures. In the arrested portion the ridges cease to recede from the axis of upheaval, while in the other portion where upheaval goes on, the ridges continue to recede. By the fracture the ridge is broken, and by the recession of one part and non-recession of the other, the ridge is faulted in such a manner as to give it the appearance of a "lateral displacement," and such phenomena have thus been erroneously explained.

The following law was stated. Where a monoclinal ridge is broken and one portion carried farther back from the axis of upheaval than the other, the faulting is by vertical upheaval, though the ridges appear to be horizontally displaced.

Mr. POWELL then explained the effect of a fault oblique to the axis of upheaval; he then explained the effect of two oblique faults crossing each other, in all these cases illustrating from examples in nature. Then he further explained how a ridge that was simple togopographically might be compound geologically, being composed in different parts of beds of different geological horizons.

In the examples considered, those changes in monoclinal ridges were produced by faults transverse or oblique to the dip of the beds or the axis of upheaval. Another series of changes are produced by faults parallel or nearly so to the axis of upheaval. On either flank of an anticlinal or in a monoclinal, there is a belt of dipping rocks, which may be denominated as the zone of flexure. It is often observed in nature that displacement proceeds as flexure along such a zone for a time, and subsequently displacement by flexure ceases, but is continued by faulting, so that a plane of fracture is found to run along a zone of flexure. If the plane of fracture is on the side of the zone of flexure nearer the axis of upheaval, the ridges composed of beds of the lower geological horizon are lost; if the plane of fracture is on the side of the zone of flexure farther from the axis of upheaval, the ridges composed of the beds of the higher geological horizon disappear; if the plane of fracture is midway in the zone of flexure, the middle ridges disappear.

Often the plane of fracture meanders through the zone of flexure where the ridges are more or less curved and broken, and ridges that are simple geographically, are often compound geologically.

All the monoclinal ridges thus far described, are MONOCLINAL RIDGES OF UNEQUAL DEGRADATION as distinguished from MONOCLINAL RIDGE OF DISPLACEMENT. The latter have a very different genesis from the former. A long fracture is produced in the sedimentary beds involved, and on one side of this line the beds are upheaved—tilted up—so as to produce a ridge with the beds all dipping in one direction. Often such upheavals are of great magnitude, producing important ranges. Such a ridge is produced directly by upheaval, and all ensuing degradation serves but to obscure the ridge-like character of the mass. Many of the ranges of the Desert System and Basin System are of this character.

The whole subject was illustrated by drawings on the blackboard.

From the facts presented, the following deductions were drawn:—

I. *Displacement by flexure is very slow, and where it appears as upheaval in relation to the level of the sea is little faster than atmospheric degradation.*

II. *Displacement by faulting is very slow, and where it ap-*

pears as upheaval it is but little faster than atmospheric degradation, but such faulting may be paroxysmal and intermittent.

III. *Faults discovered by the position of monoclinal ridges have been erroneously explained as "lateral faults." The position of the ridges is in fact due to vertical faults by which two parts of the same flexure are differentiated, the region having the greatest amplitude of flexure having its monoclinal ridges curved farther back from the axis of upheaval.*

IV. *If the rate of upheaval is greatly accelerated, the rate of degradation is greatly accelerated. If in two parts of an anticlinal upheaval, the one has an upheaval exhibiting* 10,000 *feet, the other* 20,000 *feet, the latter, celeris paribus, will have a general surface but slightly elevated above the former. In other words, the rate of degradation is chiefly determined by the rate of displacement.*

Mr. C. E. Dutton complimented Mr. Powell on the character of his work, and on the interesting results obtained by him; and proceeded to remark on some of the peculiar features of the geology exhibited in the *Colorado* region. Not only were these features on a scale perhaps nowhere else exhibited—as, for example, in the range of palpable denudation and erosion, in the channelled cañons extending for hundreds of miles, and as much as a mile deep or even more, in the great faults of several thousand feet displacement, reaching for several hundred miles, etc.; but the features were more exposed to observation, being less disguised superficially by vegetation, etc, as if here the geologist had found nature in her nakedness. Hence this great region was pre-eminently favorable to geologic study, and permitted results to be arrived at which elsewhere would require a far longer and more tedious process of exploration.

Mr. W. B. Taylor remarked that the results brought to view by Mr. Powell in his extensive researches and ingenious expositions, appeared indirectly to have quite an important bearing on the much disputed question of the thickness of the earth's crust. He (the speaker) had himself been disposed for some time past to abandon the idea of a large amount of solidity or rigidity in the earth, as supported by the plausible mathematical argument of the distinguished physicist and mathematician, Mr. Hopkins, and to recur to the earlier conception of a very thin crust resting upon or enveloping a molten and fluid spheroid. Every *geologi-*

cal consideration with which he was acquainted seemed to him to conspire in confirming this supposition with a cumulative aggregate of force.

The single fact of stratigraphical folds and faults on so grand a scale as here displayed, looked almost like a crucial test of the hypothesis. That faults continuous for one or two hundred miles, with a throw amounting in some places to several thousand feet, could take place in a solid mass, or even in a terrestrial crust indefinitely deeper than such amount of slip, seemed contrary to all known principles of physics. As this is a subject of great interest and of some "perplexity," Mr. TAYLOR said that he would take the liberty of here personally appealing to Mr. DUTTON, as a very able supporter of the Hopkins theory, to help one of the two out of this dilemma.

Mr. C. E. DUTTON replied that there were subjects on which he felt constrained to maintain a perfectly uncommitting silence; subjects of such intrinsic difficulty that he could only reserve a suspended judgment or conception; and he frankly admitted that this matter of "faults" was one of them. He did not, however, thence feel required to adopt an hypothesis which to him presented many grave objections, and which seemed in many other respects so inadequate. The phenomena of volcanoes, for example, were, he believed, generally held to be quite insufficiently explained by a thin shell; while the chemical or hydrothermal theory was much more satisfactory. This was explained by Mr. DUTTON at some length.

Mr. C. A. SCHOTT remarked, that, inasmuch as the precession of the equinoxes was an unquestionable fact, and as HOPKINS and THOMSON had shown that this was physically incompatible with a fluid mass of rotation circumscribed by a yielding shell, a solidified mass of considerable equatorial rigidity seemed to be necessarily required, and therefore established.

Mr. TAYLOR in reply said, that, first, with regard to the subject of volcanoes, he thought that if the assumption of a thin crust did not sufficiently account for all the phenomena, it was at least not incompatible with them. Taking, for example, the difficulty often urged of the great inequality of hydrostatic column and

pressure sometimes exhibited in volcanoes not very far distant from each other, he did not see why obstructions or even irregular "cellular" conditions were not quite as reconcilable with a thin crust, as with a very thick or solid one. He considered the chemical and hydrothermal theories as supported by very slender probabilities, and as having altogether the character of *pro re nata* speculations.

Secondly, with regard to the "precession" argument, he had gradually come to the conviction that we have not at present the physical data to permit a true mathematical solution of the problem; and that, however refined and ingenious these attempted mathematical investigations, they are entirely inconclusive. This appears to be the judgment of Gen. BARNARD in his Memoir on the subject, published in the Smithsonian Contributions (vol. 19). The argument, in short, when analyzed, is found to be both irrelevant and fatally supererogatory; it proves entirely too much. Thus Sir WILLIAM THOMSON, in reënforcing the view of Mr. HOPKINS, requiring as a minimum thickness 1000 miles, has gone very much further, and has reached the conclusion that if the earth were a globe of solid steel, it would be subject to solar and lunar tides, at least half as large as if formed entirely of water; and that, accordingly, with even this rigidity, the precession and nutation would not be more than three-fifths of the amount required by observation, which is that due to *perfect* rigidity. Is not this manifestly an impossible condition?

Now it must not be forgotten here that the conclusion reached by Prof. THOMSON is absolutely incompatible with the same eminent geometer's discussion of the present interior heat of our planet, in his most able Memoir "On the Secular Cooling of the Earth," published only a year earlier. The truth is that in any very large mass of matter, "solidity" becomes a merely relative term—cohesion exerting its bond only between adjacent molecules with a fixed limit of force—while gravity, the weakest of natural forces, by pervading all thicknesses, becomes indefinitely preponderant by mere accumulation. The strongest steel wire has not a tensile modulus of eighteen miles The dynamic rigidity of a rapidly rotating planet therefore, combined with even a very small amount of internal friction, must be vastly greater than any static rigidity of cohesion as known to us could possibly be. An irregular mass of granite as large as our earth would by "solid flow"

assume the spheroidal form of equilibrium almost as perfectly and as promptly as an equivalent mass of fluid.

Mr. J. W. POWELL remarked that he had studiously abstained, in every communication he had made to this Society (as the members will bear witness), from indulging in any speculations of a general or cosmological character, and had faithfully confined his remarks to a discussion of the actual facts observed, to the scale of the phenomena and of the actions involved, and to the length of time necessarily required therefor. On this mooted question—certainly not unimportant to geological principles, he had secretly his own opinions; and if he must "make a clean breast of it," he confessed that he had been slowly driven to the conviction that the shell of rock we stand and move and live upon was very thin; he was almost afraid to say how thin he thought it. Perhaps not more than fifty or sixty thousand feet. Mr. POWELL further illustrated his idea by the use of the blackboard.

Further discussion of the subject followed.

106TH MEETING. MAY 6, 1876.

The President in the Chair.

Forty members and visitors present.

Mr. J. J. WOODWARD made preliminary remarks on

THE USE OF PHOTOGRAPHY IN CONNECTION WITH THE MICROMETER MEASUREMENT OF BLOOD CORPUSCLES.

Mr. HORACE CAPRON read a paper on

JAPAN,

giving an account of his reception, his mission and its objects, the development of productive and industrial arts in that empire, and the character and habits of the natives.

Remarks were made by Messrs. ALVORD, HILGARD, and DUTTON.

Mr. G. K. GILBERT made a communication on

THE DISTRIBUTION OF THERMAL SPRINGS IN THE UNITED STATES.

107TH MEETING. MAY 20, 1876.

The President in the Chair.

Thirty-four members and visitors present

Mr. JOSEPH HENRY exhibited and described one of

CROOKE'S RADIOMETERS.

He had, however, only received it the day before, and had not had an opportunity of making any experiments with it except those of the most obvious character. His object in bringing it forward at this time was that it would interest the Society, and he would probably be absent during the remainder of the session. He thought Mr. CROOKES, like GALVANI, had commenced to investigate a phenomenon of which the cause was easily recognized, and ended in discovering a fact of great perplexity, of which the rationale, he thought, was not readily understood; that it was not a case of simple collision of elastic bodies in accordance with the dynamic theory of gases, since the revolution was apparently in the wrong direction.

Mr. W. B. TAYLOR remarked that in regard to this very striking experiment, he did not see how there could be any serious doubt as to the character of the energy displayed. All suggestions as to the possible action of "Light" in the case appeared to him to quite ignore the only just conception or rational definition of that agency admissible—as the medium of *vision*. "Light" is obviously a subjective or physiological phenomenon, and not a dynamic one; merely a peculiar impression of wave-periodicity on a highly specialized nerve structure. And while we have its various affections in phosphorescence, fluorescence, polarization, absorption, and measure its intensity in photometry, yet these effects all relate to, and terminate in, the seeing eye. And without an eye, there can be no such thing as "light."

The minute wave-motions which result from atomic periods have, as we all know, a far wider range than that recognized by sight; and these wave motions are capable, as all know, of exerting a dynamic effect which may be indefinitely accumulated. But there is no accumulation of "light." Hence, to speak of the "mechanical equivalent" of light, or of determining the pressure of a luminous ray, or of "weighing sunshine," is simply to misuse words, and to confound incongruous ideas.

As to the interpretation of this curious and undoubtedly abstruse phenomenon, Mr. TAYLOR thought it to be tolerably well established that neither attenuated air-currents, nor evaporation, nor "electricity" had any agency in the case; but that the explanation given by Prof. DEWAR, of Edinburgh, and by Mr. STONEY, was undoubtedly the true one, namely, that the blackened side of the disks absorbing more heat-motion than the unblackened side, communicated more reactionary motion to the extremely rarefied air in the small glass chamber. Mr. CROOKES undoubtedly deserved great credit for the ingenuity and patience displayed in varying and testing the experiments. The name "radiometer," however, he thought unhappily chosen, as it suggested or countenanced the idea that the vanes were acted upon by the impulsion of radiation; whereas the instrument was really a differential absorption-scope.

Mr. S. NEWCOMB suggested that this part of the question could very easily be tested by arresting the motion of the vanes (either by a strong magnet or otherwise), throwing a strong beam of radiant heat upon the blackened disk, then suddenly cutting off entirely the source of radiation and removing the detent, to observe whether any motion occurred. If it should, then evidently this could not be the effect of direct impulse or momentum from the radiation.

As to the explanation alluded to by Mr. TAYLOR, the speaker understood that Mr. STONEY'S theory, as set forth in the Philosophical Magazine for March and April last, involved a reaction between the heated disk and the side of the glass chamber, through the medium of the residual air; this reaction commencing when the air becomes so rare that the molecules rebounding from the surface of the vane reach the enclosing chamber without meeting other molecules. If this explanation be correct, it would follow

that in a vacuum chamber, above a certain size, the balance disk-arms should not revolve without increasing the rarefaction.

Mr. TAYLOR replied that he so understood Mr. STONEY'S exposition, though he had neglected to state it fully.

Mr. F. F. JUDD made a communication on

THE ADIRONDACK WATER-SHED,

referring particularly to the transformation of the lakes of that region into reservoirs, thus checking the spring freshets in the Hudson River and its tributaries, and providing a supply of water in the season of droughts.

Remarks were made by Mr. HILGARD, by Mr. HOLDEN in reference to a Report made by Maj. FARQUHAR, of the U. S. Engineers, on similar measures for the Mississippi River, and by Mr. ABBE on the constructions for the same purpose in the Rhine and other European rivers.

Mr. F. V. GREENE read a paper on

THE DEVIATIONS OF THE PLUMB-LINE AS DETERMINED IN THE SURVEY OF THE 49TH PARALLEL OF LATITUDE.

(This paper is published in full in No. XL. of the Printed Papers of the Essayons Club of the Corps of Engineers.)

(ABSTRACT.)

The object of this paper was to investigate the causes of the discrepancies between the astronomical and geodetic determination of points on the 49th parallel of latitude; and it was illustrated by maps and diagrams showing the topography and geology, and the deviation at each station.

This parallel (the International Boundary line) was determined and marked for 853 32 miles, from the Lake of tne Woods to the Rocky Mountains: the basis of the determination rested on 47 astronomical stations, at each of which the latitnde was observed with the Zenith Telescope. Adjacent astronomical stations were connected, and intermediate points of the parallel determined, by the method of "tangents and offsets." These operations were explained sufficiently in detail to show that the greatest uncertainty in the astronomical and geodetical determinations at any one point could not exceed 40 feet, or 0".4. Yet the observed discrepancies had an average value of 2".15, a maximum of 7".28

and a minimum of 0″.15. The first and last stations differed by 0″.05. The extreme range of all the stations was 13″.89.

An effort was then made to see how far these discrepancies could be accounted for by the local attractions of irregular masses above the surface of the earth in the vicinity.

The formulæ given by Clarke in the "Principal Triangulation of the Ordnance Survey" were applied in connection with contoured maps, to calculate the deflections due to the superincumbent masses.

The difference between the observed and calculated deflections had an average value of 1″.47; *i. e.* about two-thirds of the observed deflection could not be accounted for by the topography and must be due to causes underground.

General geological maps were also exhibited, and it was suggested that a large and quite regular deviation at all the stations for 150 miles in the vicinity of the Red River was due to the difference in density of the secondary and tertiary formations crossing the line, obliquely, in that neighborhood.

A short statement of the history of investigations on this subject, and its bearing on the determination of the figure of the earth, concluded the paper.

Mr. HILGARD spoke of the large deviations found near the Pacific coast in the operations of the U. S. Coast Survey.

108TH MEETING. JUNE 3, 1876.

The President in the Chair.

Thirty-five members and visitors present.

Mr E. P. LULL gave an account of the country traversed by the late expedition under his command for the investigation of the route for

THE INTEROCEANIC CANAL THROUGH NICARAGUA.

Mr. J. W. POWELL read

A BIOGRAPHICAL NOTICE OF MR. A. R. MARVINE.

(This notice is published in Appendix X of this Bulletin.)

Mr. T. N. GILL, in behalf of a committee appointed for the purpose, reported the following resolutions, which were unanimously adopted.

Whereas, ARCHIBALD R. MARVINE, late a member of the Philosophical Society of Washington, has been taken from us by death, and

Whereas, Our late associate by thorough preparation, industry, and ability had, though young, made interesting and valuable contributions to human knowledge by original research in geology, and thus gave promise of a fruitful scientific career:

Resolved, That we mourn his death as a loss to our Society and to the scientific world.

Resolved, That we, as members of this Society, tender to his family our sympathy with them in their sorrow.

Resolved, That a copy of these resolutions be transmitted to his family by the Secretary.

109TH MEETING. JUNE 17, 1876.

Vice-President TAYLOR in the Chair.

Twenty members and visitors present.

The Chair announced the election of Mr. C. ABBE as Treasurer in the place of Mr. PARKER, resigned, and of Mr. ASAPH HALL and Mr. M. C. MEIGS to fill vacancies in the General Committee occasioned by the resignations of Mr. PARKER and Mr. DUTTON.

Mr. THOMAS ANTISELL, by request, gave an account of his visit to Japan, describing the geographical and geological features of those islands, their meteorology and climate, the origin and ethnology of the people, their history and social condition, their occupations and progress in industrial arts.

110TH MEETING. OCTOBER 7, 1876.

The President in the Chair.

Thirty-one members and visitors present.

Mr. E. B. ELLIOTT made some preliminary remarks on

"FORCE AND MOMENTUM,"

and on a system of measures depending on the earth's polar radius.

A conversational discussion followed, in which Messrs. MASON, WOODWARD, HILGARD, and TAYLOR participated.

Mr. E. S. HOLDEN spoke of observations of the sun at the U. S. Naval Observatory in search of the supposed interior planet Vulcan. These were made for three successive days, chiefly with the comet-seeker, occasionally with the nine-inch refractor, but without success.

Messrs. NEWCOMB and TAYLOR discussed the theory of Vulcan, and the probabilities of the existence of such a planet.

Mr. JOSEPH HENRY described recent experiments under the direction of the U. S. Lighthouse Board, particularly on the combination of sounds produced by two syrens of the same pitch, and which were heard at a much greater distance than one alone; and the construction of a buoy so as to give sounds by the force of the waves.

Mr. J. W. POWELL spoke of the requirement by geologists of more time, less thickness of the earth's crust, and more contraction than physicists were ready to allow.

Messrs. NEWCOMB, HILGARD, TAYLOR, and GILL participated in the discussion which followed.

111TH MEETING. OCTOBER 21, 1876.

Vice-President WELLING in the Chair.

Twenty-two members and visitors present.

Mr. E. B. ELLIOTT made some remarks on

MONETARY STANDARDS;

referring to recent action of France, Germany, Austria, and Spain, in limiting the coinage of silver, and restricting the use of silver coins mainly to a subsidiary currency.

Some discussion followed, chiefly with regard to the large amount of silver required by Germany at the present time, in which Messrs. ABBE, NEWCOMB, and WELLING participated.

112TH MEETING. SIXTH ANNUAL MEETING. NOVEMBER 4, 1876.

Vice-President HILGARD in the Chair.

Twenty-two members present.

The election of Lieut. Commander FRANCIS M. GREEN, U. S. Navy, as a member of the Society, was announced, and the names of members elected since the last annual meeting were read.

The following officers of the Society were elected for the ensuing year:—

President,	JOSEPH HENRY.
Vice-Presidents,	J. K. BARNES, W. B. TAYLOR, J. E. HILGARD, J. C. WELLING.
Treasurer,	CLEVELAND ABBE.
Secretaries,	J. H. C. COFFIN, T. N. GILL.

MEMBERS OF THE GENERAL COMMITTEE.

S. F. BAIRD,
E. B. ELLIOTT,
ASAPH HALL,
N. S. LINCOLN,
S. NEWCOMB,
O. M. POE,
C. A. SCHOTT,
J. M. TONER,
J. J. WOODWARD.

113TH MEETING. NOVEMBER 18, 1876.

The President in the Chair.

Thirty-one members and visitors present.

The election of Mr. LESTER F. WARD as a member of the Society was announced.

The President presented to the notice of the Society a specimen of fire-proof

PAPER MADE OF ASBESTOS.

He also had read a letter received from Mr. CHARLES G. BOERNER, of Vevay, Switzerland Co., Indiana, describing a remarkable

SHOWER OF THE ROCKY-MOUNTAIN GRASSHOPPERS,

Caloptenus spretus, and *Caloptenus femur-rubrum*, at that place on the 13th of November last at 6½ P. M., continuing till 8 P. M. They came in immense numbers, filling the air and covering the ground, in some places densely, adhering tenaciously to clothing on which they lighted, but not lodging on trees or shrubbery.

He speaks of the day as having been one of unusual mildness for this season of the year. The wind oscillated between the southwest and west, with a velocity of two to three miles an hour; at 9 P. M. blowing in moderate gusts from the west.

Mr. GILL recognized a specimen presented as a *Caloptenus spretus*.

Mr. E. B. ELLIOTT made a communication on

MUTUAL RELATIONS OF GOLD AND SILVER, AND OF PRICES OF COMMODITIES;

presenting the following table, condensed from the "London Economist," giving the wholesale prices in gold of leading commodities in London and Manchester at different epochs compared with the average price in 1845 to 1850, just prior to the discovery and full working of the gold mines of California and Australia.

The commodities were:—

No. 1.	Coffee.	No. 17, 18.	Flax & Hemp.	No. 34.	Copper.
2–5.	Sugar.	19–22.	Sheep's Wool.	35, 36.	Iron.
6.	Tea.	24.	Indigo.	37.	Lead.
7.	Tobacco.	25–27.	Oils.	39.	Tin.
9.	Wheat.	28, 29.	Timber.	42.	Cotton Wool. (Pernambuco only).
10–13.	Butchers' Meat.	30.	Tallow.	43.	Cotton Yarn.
15.	Cotton.	31.	Leather.	44, 45.	Cotton Cloth.
16.	Silk, raw.				

Comparative Statement of Wholesale Prices in London and Manchester for a series of years, to wit: The averages for the six years 1845–50, *and selected years from* 1851 *to Jan.* 1*st*, 1876, *from the "London Economist," under the head of* Commercial History and Review, March 11th, 1876, page 37.

		Total Index No.	Proportionate Nos. compared with that for 1845–1850.
1845–50.	6 years' average.	2200.	100.
1851.	1 January.	2293.	104.2
1853.	1 July.	2361.	107.3
1857.	1 July.	2996.	136.2
1858.	1 January.	2612.	118.7
1865.	1 January.	3575.	162.5
1866.	1 January.	3567.	162.0
1867.	1 January.	3024.	137.5
1868.	1 January.	2682.	121.9
1869.	1 January.	2666.	121.2
1870.	1 January.	2682.	122.2
1871.	1 January.	2590.	117.7
1872.	1 January.	2835.	128.9
1873.	1 January.	2947.	134.0
1873.	1 July.	2914.	132.5
1874.	1 January.	2891.	131.4
1874.	1 July.	2779.	126.3
1875.	1 January.	2778.	126.3
1875.	1 July.	2692.	122.4
1876.	1 January.	2711.	123.2

The table shows an increase in 1858 of 18 or 19 per cent.; in 1868, 1869, 1870, and 1871, years of minimum prices, an increase of from 18 to 22 per cent.; in 1875, another year of minimum prices, an increase of 22 to 23 per cent. above the average prices in 1845–1850. In some intervening years there were remarkable fluctuations upwards, notably in 1857, 1865, 1866, and 1873.

Mr. Elliott argued that these fluctuations were chiefly changes in the values of commodities due to excessive speculative activity, while the value of gold continued nearly uniform, as indicated by the years of normal or minimum prices. He claimed, however, that the rise from 1845–50 to 1858 was due to the greatly increased production of gold, and consequent diminution of its value.

He gave also a comparison of the relative prices of gold and silver, showing a relative advance of the latter from 1845–50 to

1859 of about 5 per cent., a return to the same value in 1873, and then a decline, very largely augmented in 1875 and 1876, and reaching a minimum price in July, 1876, of about 79 per cent., compared with the relative value in 1845–50, being a decline of 21 per cent. from that value. From that point, however, it has again advanced, so that at the present time the relative value of silver to gold is only 10½ per cent. below that of 1845–50.

Mr. EMIL BESSELS made a communication on

THE LATE ENGLISH POLAR EXPEDITION,

showing the positions reached, the temperatures, limits of ice and open water observed, and comparing them with the results of other expeditions. He demonstrated that the meteorological features exhibited at the winter-quarters of the Englishmen were anomalous, and gave some results of investigations unpublished yet, being on the change of the temperature of the air with the latitude. He dwelt especially on the tides, stating that the tidal wave reaching the winter-quarters of the "Alert" and "Discovery" is not the Pacific wave, as stated in the report of Captain NARES, but one of different origin.

Mr. DALL stated that, if the assertion of Houghton, who had examined the tidal records of the English exploring vessels at Point Barrow, was correct, the tides there were of a simple semi-diurnal character and quite different from those of Behring Sea or of the northwest coast of America. He considered it probable that the Arctic basin to the westward of the Parry Archipelago has a tide of its own, which, however, was not to be confounded with that experienced by the Arctic explorers to the eastward, and which Dr. Bessels had very clearly shown to be derived from the North Atlantic wave, and in all probability propagated around the northern coast of Greenland. The speaker considered it as almost certain that no tidal wave was propagated northward through the shallow strait of Behring; particularly as the observations at St. Paul Island and in Norton Sound indicated that Behring Sea itself has a peculiar tide which is distinct from that observed to the southward of the Aleutian Islands.

114TH MEETING. DECEMBER 2, 1876.

The President in the Chair.

Forty-two members and visitors present.

The election of Mr. DAVID SMITH, Engineer U. S. Navy, and Mr. MARCUS BAKER, of the U. S. Coast Survey, as members of the Society, was announced.

Mr. GARRICK MALLERY read a paper on

A CALENDAR OF THE DAKOTA INDIANS.

(*A photolithograph of the Chart exhibited, with a detailed description and translation, is to be published in the Bulletin of the U. S. Geological and Geographical Survey of the Territories*, vol. iii. No. 1, in press.)

(ABSTRACT.)

Painted narratives of tribal and personal events, delineated by representations of men and animals and other figures, on hides or bark, are common among the nomadic tribes of North America. The Eastern Algonquins also used "wampum," an arrangement of stringed beads fashioned from shells of different colors, to note battles, treaties, and other occurrences of moment, their devices being generally mnemonic only, and seldom symbolic. The Pueblos figured their histories on tablets of wood; and the Aztecs and Toltecs have left elaborate records in picture writing. It is, however, submitted that in the similar productions of all of these peoples, before discovered, the obvious intent was either historical or biographical, that is, to chronicle events as such, and there was no apparent design to symbolize occurrences selected without reference to their intrinsic importance or connection with each other, but merely because they occurred within successive intervals of time, and to arrange them in an orderly form, specially convenient for use as a calendar and valuable for no other purpose. The chart now exhibited appears to be an attempt, before unsuspected, on the part of the Northwestern Indians, to form for themselves a system of chronology.

The copy brought by the writer from the Sioux country in November, 1876, is in two colors, black and red, the symbols covering a yard square of cloth, and purports to be a fac-simile of the original, which was drawn by and is believed to be still in the possession of Lone Dog, an aged Indian belonging to the Yanktonai tribe of the Dakotas, who, in the autumn of 1876, was near Fort Peck, Montana Territory, and in the then condition of the region was not directly accessible. The authenticity of the document was verified by separate examination through

different interpreters of the most intelligent Indians at Fort Rice, Dakota Territory, and other posts and agencies, eliciting a nearly complete explanation of the symbols, and the following account:—Lone Dog has been, ever since his youth, charged with the duty of deciding upon some event or circumstance which should distinguish each year as it passed, and when such decision was made, he marked what was considered by him its appropriate symbol, upon a buffalo robe kept by himself for the purpose, then calling together a number of the Dakotas, without regard to tribes, explained to them the symbol, and what it represented. This was done annually and formally, but it is understood that the robe was also at other convenient times exhibited to other Indians, who were thus taught the meaning and use of the signs as designating the years. The copy actually discovered was obtained from Basil Clèment, a half-breed interpreter, living in 1876 at Little Bend near Fort Sully, D. T., and, it is understood, is a duplicate of a copy, taken in 1870 or 1871 from Lone Dog's tribe, of its condition at that time. This copy the writer was informed was in the possession of Blue Thunder, a member of the Blackfoot tribe of the Dakotas, who was in October, 1876, at Standing Rock agency, D. T.

The symbols on the chart are seventy-one in number, and designate the seventy-one years, commencing with the winter of A. D. 1799–1800. It is not yet ascertained whether Lone Dog had a predecessor in his work from whom he received the earlier symbols, or whether the essay at chronological tables being first started when he reached manhood, he gathered the traditions from his elders, and himself distinguished by signs a number of years then past.

A suspicion naturally arises that intercourse with missionaries and other whites first gave the Dakotas some idea of dates, and awakened in them a sense of want in that direction. The fact that the calendar begins with a time nearly identical with the first year of the present century by our computation, may be due to such influence, or may be a mere coincidence. If missionaries or traders started any plan of chronology, it is remarkable that they did not suggest one similar to that common among themselves, that is, by counting in numbers backward and forward from an era. The chart, however, shows nothing of this nature. The earliest symbol merely represents the killing of a small number of Dakotas by their enemies, an event of frequent recurrence, and neither so momentous nor interesting as many others of the seventy-one recorded, more than one of which, indeed, might well have been selected as a notable fixed point, before and after which simple arithmetical notation could have been used to mark the years. The plan actually adopted, to individualize each year by a specific recorded symbol or year-totem, according to the decision of a single designated officer and

his successors, whereby confusion was prevented, was both original and ingenious, showing more of scientific method than has often been attributed to the nomadic tribes. This is also true of the practical arrangement, by which the distinctly separate characters follow from right to left in an outward spiral, starting from a central point, allowing every date to be determined by counting backward or forward from any other that might be known, yet wholly dispensing with the use of numbers to note the years. It seems unlikely that this device, so different from that used by the whites, should have been prompted by them.

A number of the designs on the chart are purely arbitrary, but the greater part are graphic illustrations, which, indeed, are used whenever the nature of the event allowed. The appearance of the smallpox (in 1801), represented by a man figure covered with red blotches; the first capture of wild horses (in 1812), by a lasso; the great meteoric shower of Nov. 12, 1833, and several other symbols, are so suggestive as not to require any assistance in their interpretation. In the method of selecting occurrences it is clear that the criterion was not their own importance, but their character for incident or particularity in connection also with notoriety. A good example is in the signs for 1806 and 1808. In the first, an Arickaree is killed by a Sioux as he is in the act of shooting an eagle, and in the latter the Sioux who killed him is himself killed by the Arickarees. War then raging between the Dakotas or Sioux, and several tribes, doubtless many on both sides were killed in each of these years; but there was some incident and probably much gossip about the Ree, who was shot, just when in fancied security he was bringing down an eagle, and whose death was avenged by his brethren the second year afterwards: hence the selection of these trivial occurrences. It would, indeed, have been impossible to have graphically distinguished by separate signs the many battles, treaties, stampedings of horses, eventful hunts, etc., so most of them were omitted, and other commonly known incidents, of greater individuality, and better adapted for portrayal, were often taken for the calendar, though they were of absolutely no national or tribal consequence. A notable feature of the chart is the effort of the author to make each symbol, emblem, or character distinguishable from all the others; and he is unsuccessful in but two instances, while even in those the error appears due to the copyist. This feature is not observed in the pictured histories or biographies found, in which repetitions both of figures and subjects are frequent.

Mr. Hilgard remarked on the custom, even in the most highly civilized nations, of fixing dates by special or notable events.

Mr. POWELL referred to historical charts among the Pueblos, the interpretations of some portions having been confirmed by Spanish annals. Pictorial writings have been discovered in Mexico and South America; and SCHOOLCRAFT has published such writings, found among the Indians of North America. Mr. POWELL spoke of the mythological character of all these writings, and the absence of anything mythological, except in one instance in the drawings exhibited by Mr. MALLERY. From his observations of Indian custom he did not understand how there could be so many symbols for names of individuals, nor the picturing of the total eclipse of the sun in 1869 as it actually appears, instead of the usual mythological representation of a monster devouring the sun.

Mr. ABBE remarked that he must differ from Mr. POWELL'S opinion that it is improbable that any Indians would depict the solar eclipse of 1869 by any other than a mythological symbol. He himself had in August of that year led a party of seven young men to Fort Dakota, or Sioux Falls City, in order to observe that eclipse, which purpose was successfully accomplished.

During their week's stay at that place they had explained the eclipse to numerous Sioux, and probably an hundred of those Indians had occasion to realize that white men knew of the approaching event, and that to them the phenomenon was no mystery. On the day itself a party of six or seven warriors lingered about the camp until the progress of the eclipse became evident to the naked eye, when, all their doubts being dispelled, they departed with a shrug of the shoulder and "Ugh! bad medicine."

The eclipse, as seen in that high latitude and elevated region, where the Dakotas roam, was impressive and beautiful in the highest degree. The stars, which were thus made visible in the daytime, have been deemed by the Indian chronologist worthy of marking the year 1869.

Mr. ABBE further remarked that the whole of this Dakota record seemed to him to be eminently practical and truthful, and free from fancy or mythology; as we might, indeed, expect it to be when we consider that the compiler was one of the original geniuses of his race, an innovator upon the ordinary customs of the Indians, although he may have heard of similar representations among the Aztecs or the whites. The meteoric shower in

1833, the comet or meteor in 1821, the horseshoes, lassoes, diseases, etc., are here all truthfully presented in an admirable matter-of-fact spirit.

He expressed the hope that our western explorers will spare no pains to see and examine the original chart, and learn from its possessor all they can respecting its origin and design.

Mr. PARKER gave instances of coincidences of words, ideas, and superstitions of the Chinese and the American Indians, remarking that the investigation of such coincidences might lead to conclusions respecting the origin of the Indians.

Mr. GILL was at a loss to understand why the chart exhibited should begin with the year 1800.

Further remarks were made by Mr. GILBERT and Mr. DALL.

Mr. ASAPH HALL made a communication on

THE APPEARANCE OF SATURN'S RINGS.

(ABSTRACT.)

In this paper Mr. HALL gave some account of the difficulty of correctly delineating the shadows and phenomena attending the appearance of the rings of Saturn. He has observed the satellites of this planet with the 26-inch refractor of the Naval Observatory during the summers of 1875 and 1876; and has paid particular attention to the appearance of the rings since September, 1875. He stated that he has never seen the notch in the outline of the shadow of the ball on the rings, which is so marked a feature in the picture of this planet made by Mr. TROUVELOT at Cambridge, Mass., in December, 1874. This outline has always appeared as having a regular and continuous curvature. Neither has he ever seen the jagged appearance in the division of the rings near the ansæ.

He then spoke of the following phenomena:—

(1) Although the principal division of the rings has been easily visible until the present time, no other divisions have been seen. Slight markings have been noticed in the rings, which may be caused by other divisions.

(2) The dusky ring has been remarkably bright during the summer of 1876.

(3) The convexity of the outline of the shadow has always been seen turned toward the ball of the planet, and not away from it, as it is usually drawn. Thinking this to be an illusion, Mr. HALL has tried various ways of dispelling it, but has never succeeded.

Remarks on this paper were made by Mr. HILGARD and Mr. HOLDEN.

115TH MEETING. DECEMBER 16, 1876.

The President in the Chair.

Thirty-seven members and visitors present.

The election of Mr. CHARLES ABIATHER WHITE as a member of the Society was announced.

Dr. J. J. SYLVESTER, of the Johns Hopkins Institute, Baltimore, made a communication on

SOME RECENT INVESTIGATIONS ON THE THEORY OF INVARIANTS,

giving an account of what had been done in that special field of research, and of the advances he himself had been able to accomplish.

Mr. J. M. TONER read a paper on

THE BURNING OF THEATRES AND PUBLIC HALLS,

with reflections on some of the causes of the great mortality occasionally attending such fires, and suggestions for improved security to life; with a chronological list of theatres and other public edifices burned.

(*This paper has been printed as a* 12*mo. pamphlet, Washington, D. C.*, 1876.)

Mr. E. S. HOLDEN presented a paper

ON REFERENCE CATALOGUES OF ASTRONOMICAL PAPERS AND MEMOIRS.

(ABSTRACT.)

I have received from my friend E. B. KNOBEL, Esq., F.R.A.S., of London, an advance copy of his Reference Catalogue of Astronomical Papers and Researches, reprinted from the Monthly Notices of the Royal Astronomical Society for Nov. 1876, pp. 365 *et seq.* I desire to call the attention of the Astronomers and others of the Society to this paper on account of its importance, but specially to make a few remarks upon its accuracy, which I have been able to test; as well as to say a few words on Astronomical Bibliography in general.

The subject of Scientific Bibliography in general, has received much attention, and we have now at our command many works, some of which are of great importance.

In Astronomy I mention the more important in order of publication, omitting of course in such an enumeration indices to periodical literature which are published as supplements to the periodicals in question, even when they are so valuable as the indices to the Astronomische Nachrichten, the Coast Survey Reports, etc.

WEIDLER, J. F.: *Bibliographia Astronomica, etc.* 1755. 8vo.

This work was undertaken at the instance of DE L'ISLE and is dedicated to him, and it formed the basis of the Bibliography of LALANDE. I am not aware of the method of arrangement adopted.

SCHEIBEL, J. E.: *Astronomische Bibliographie.* 1st part 1784, 2d part 1786, 3d part with Appendices to parts 1 and 2, 1789–1798. 8vo.

I have not seen this work, which is not in the library of Congress at present, but from the Introduction to LALANDE's Bibliography I learn that it had 800 pages and extended only to 1650, and that the notes accompanying each entry were very full. It must be quite complete to 1650, for in the year 1591, in which LALANDE has three entries, SCHEIBEL has twenty. I infer it is arranged chronologically.

LALANDE, J. DE: *Bibliographie Astronomique, etc.* 1803. 4to.

This work, in which the bibliographical part occupies above 600 quarto pages, is founded on WEIDLER's book, already mentioned. The titles are arranged chronologically throughout, no division into subjects being attempted, except in the index, which is arranged by subjects, and which could be improved. In very many cases LALANDE has added to the title an abstract of the contents of the book. Periodicals, as the *Philosophical Transactions*, for example, have the separate volumes indexed under the year in which they were published.

REUSS, J. D.: *Repertorium Commentationum, etc.* Vol. V. *Astronomia.* 1804. 4to.

REUSS confined his work to the indexing of the Transactions of Societies, but these are catalogued from their commencement to about 1804 (for Astronomy), so that with the Royal Society's Catalogue, all Transactions are indexed up to 1863. In REUSS, however, the arrangement is chronological under various topics, and not by authors; and some subjects, as Nebulæ for example, are not given.

YOUNG, THOS.: *A course of Lectures on Natural Philosophy, etc.* 2 vols. 1807. 4to.; Vol. II., pp. 87 to 520, consists of a "Systematic Catalogue of Works relating to Natural Philosophy [Astronomy], etc."

The comments by the distinguished author of this Bibliography will always render this work classic. In some of the departments of astronomy the catalogue of 53 pages is almost complete to 1800. A most complete index renders it easy of consultation.

SOHNCKE, L. A.: *Bibliotheca mathematica: Verzeichniss der Bücher über . . . Astronomie welche in Deutschland und dem Auslande vom Jahre* 1830 *bis* 1854 *erschienen sind.* 1854. 8vo.

This work is principally concerned with mathematics, but it includes a section on Astronomy and Geodesy. The order of arrangement is alphabetical by authors; books and separately published memoirs are registered under authors' names. An alphabetical index of subjects is added, each subject containing authors' names and references to the main body of the work.

SCHUMACHER, H. C.: *Catalogue des Livres composant la bibliothèque de feu H. C. Schumacher.* Part I. [Mathematics, Astronomy, etc.] 1855. 8vo., pp. 147.

This is a catalogue of the best astronomical library ever possessed by a private individual, and is of value in many cases. In this connection may be mentioned similar catalogues of the libraries of astronomers, prepared by the various booksellers, as those of the libraries of ARGELANDER, OLUFSEN, etc., and also the periodical subject catalogues of FRIEDLANDER, ASHER & Co., KÖHLER and others. These are often quite full.

OTTO V. STRUVE: *Catalogus Librorum in Bibliotheca Speculæ Pulcovensis.* 8vo. 1858.

This consists of an elaborate catalogue of all the works contained in the unrivalled astronomical library of the Pulkova Observatory up to 1858. It comprises—

1st. 4112 titles of works; 7625 volumes.

2d. 143 celestial maps, charts, etc.

3d. 14,634 smaller works, or dissertations.

In this work the memoirs are arranged by subjects and chronologically in each subject, without notes. It was edited by OTTO STRUVE with great care, and practically completes the bibliography of Astronomy, Mathematics, Geodesy, and allied sciences to the date of publication, and will long remain an acknowledged classic on its subject. It is continued in MS. at the Pulkova Observatory, and there is reason to hope for the publication of a supplement shortly.

ROYAL SOCIETY OF LONDON: *Catalogue of Scientific Papers* (1800–1863). 6 vols. 4to. 1867–1872.

This catalogue, which originated in a communication of Prof. HENRY to the British Association in 1855, is intended to serve as an Index to the Titles and Dates of Scientific Papers contained in the Transactions of Societies, Journals, and other Periodical works which have been published from the beginning of the present century to the end of 1863. No separate publications, as books, are included. It is unnecessary to say that it is practically indispensable to any course of reading. It is to be continued.

DARBOUX and HOÜEL: *Bulletin des Sciences Mathematiques et Astronomiques*. Periodical. 8vo.

This contains the full titles of books and memoirs on mathematics and astronomy, and in many cases most complete abstracts of them. It is published serially and still continues.

POGGENDORF: *Biographisch-Literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften, etc.* 2 vols. 8vo. 1863.

This work is arranged alphabetically by authors, and is intended to cover all the important publications of each author, whether published separately or in the Transactions of Societies. Further, it is intended to cover the publications of the ancients as well as of the moderns. It is extremely valuable as an extension of the Royal Society's Catalogue, and as an independent test upon it. If but one work of astronomical bibliography can be owned, this is undoubtedly the one of most general value.

WOLF, R.: *Sonnenflecken—Literatur, etc.*

In the *Astronomische Mittheilungen* of WOLF a regular record of the literature of sun-spots is kept up, and when this is eventually brought together, a very little labor will make it complete.

WOLF'S *Handbuch der Mathematik, Astronomie, etc.*, 2 vols. 8vo., 1872, contains also a great number of references arranged by subjects.

CARL, PH.: *Die Principien der Astronomische Instrumenten Kunde*. 8vo. 1863.

As an Appendix to his *Principien*, p. 161, CARL gives an exhaustive catalogue of memoirs, etc. upon micrometers and micrometer screws, of high importance. This is arranged alphabetically by authors.

CARL: *Repertorium der Cometen—Astronomie*. 8vo. Munich, 1864. pp. 377.

This work contains an account of every comet up to 1864, and besides giving the various systems of elements, etc., which have

been deduced, refers to all places in periodical and other literature where the particular comets are treated of, or where observations are given, and is a monument of industry and exactness.

WELLER: *Cometen-Literatur;* in the Anzeiger für Künde der Deutschen Vorzeit, 1857, No. 10, p. 321; No. 11, p. 359.

This work, which I have never seen, refers to the comets of 1556, 1570, and 1577.

ST. PETERSBURG ACADEMY OF SCIENCES: *Tableau géneral méthodique et alphabetique des Matières contenues dans les Publications de l'Académie Impériale des Sciences de St. Petersbourg depuis sa fondation 1re partie. Langues étrangères.* 8vo. 1874.

The subject "Astronomy" is quite full in this list, as might be expected, and it is arranged alphabetically by authors.

BELGIAN ACADEMY OF SCIENCES: *Bibliographie Académique.* 1875. 8vo.

This volume is devoted to bibliography and biography. Each member of the Academy is mentioned separately and a brief sketch of his life given, and following this is a list of his papers; *first*, those published in the volumes of the Academy; and *second*, those published elsewhere. The first list is complete; the second quite full. It includes a number of astronomical papers.

ENGELMANN, R.: *Literatur der Astronomische Nachrichten, etc.*

In his recent edition of the selected works of BESSEL, Dr. ENGELMANN has collected, after each separate subject, a list of all the publications on this subject which have appeared in the *Astronomische Nachrichten* up to the present date, the whole constituting a topical index of high value. There are now three volumes 4to. of Indices to the *Astronomische Nachricten*, very full, arranged *both* by authors and subjects.

The volume and page of ENGELMANN'S BESSEL'S Abhandlüngen, where these lists are printed, are given below.

Vol.	Page.	Subject.
I.	83.	Comets.
I.	194.	Saturn and Saturn's satellites.
I.	260.	Refraction.
I.	316.	Aberration, Nutation, and Precession.
II.	236.	Parallax of Stars.
II.	281.	Fundamental Stars; Star Catalogues and Star Charts.
II.	404.	* Mathematics.
II.	325.	Proper Motion, variable proper motion, Double Stars, *Sirius'* companion, etc.
III.	138.	Geodesy; Longitudes; measurement of an arc of meridian.
III.	286.	Pendulums, etc.; Units of mass, etc.; Terrestrial Refraction, etc.
III.	489.	Miscellaneous.

* [I may say here that the bibliography of mathematics in general is very full, but its consideration is beyond my present purpose.]

KNOBEL, E. B.: *Reference Catalogue of Astronomical Papers and Researches.* 8vo. 1876.

(Exhibited to the Society.)

Mr. KNOBEL has been for some time engaged in the formation of an index-catalogue to scientific papers, books, etc. on certain subjects of stellar astronomy which was intended to be exhaustive of their literature. The subjects chosen were: 1, Double Stars, and the theory of Binaries; 2, Variable Stars; 3, Red Stars; 4, Nebulæ and Clusters; 5, Proper motions of Stars; 6, Parallax of Stars; 7, Stellar Spectra.

The libraries of the Royal Society, of the Royal Astronomical Society, and others, were *completely* indexed as to these topics, and that of the British Museum was constantly consulted. Under each head the titles of periodicals and names of authors are given alphabetically and in the briefest way; references are made to the volume and page. The titles of books and of the most important memoirs are given in full, while the minor papers are referred to only by volume and page.

Brevity has been studied in every way. In the references to the *Astronomische Nachrichten* only the *number* is given, omitting the *column*, etc. etc. The Royal Astronomical Society's Monthly Notices are in the subject of "Double Stars," where *every* volume contains many references, only referred to generally. In other subjects where the references are more scattered, these are referred to by title and page.

Double Stars, etc.	occupies	6	pages nearly.
Variable Stars	"	4	"
Red Stars	"	1	"
Nebulæ and Clusters	"	4½	"
Proper motions	"	3½	"
Stellar Parallax, etc.	"	4	"
Star-spectra	"	2	"

In Double Stars I find more than 700 single references, and the other subjects are equally full. It is original work, the Royal Society's Catalogue being used for verification only. It is brought up to 1876, and contains above 3000 titles. I have examined that portion of Mr. KNOBEL's Catalogue which relates to *Nebulæ and Clusters* line by line, almost entry by entry, by means of a catalogue of works on the same subject which I have myself made, and I have not found in the whole list more than one or two erroneous references, and these were not such as would interfere with the finding of the paper sought for. Only one important paper is omitted, viz., D'ARREST's *Siderum Nebulosorum observationes Havnienses.* It has an *original* value which must not be overlooked. It was formed by looking through the works consulted and extracting what was required, and not by extracting references to them from known indexes, such as the Royal Society Catalogue, and therefore it may appropriately be used as a check

on all other work of the same kind. Only those who have done this nature of work know how easily errors may slip in and how omissions may occur, and the accuracy of this list shows that extraordinary care has been taken in its compilation. I have found but one misprint in it, viz., *Comptes Rendus*, vol. 28, p. 537, should be p. 573. The name of BODE has been omitted by the printer.

HOLDEN, E. S.: *Catalogue of papers and memoirs relating to Nebulæ and Clusters.* MS.

(Exhibited to the Society.)

The necessity of supplementing the Royal Society's Catalogue of Scientific Papers, which, it will be remembered, is arranged solely by authors, and only contains papers published in periodicals since 1800, and which excludes books separately published, was impressed upon me some two years ago when I was endeavoring to acquire a thorough acquaintance with the literature of a single subject—that of Nebulæ and Clusters. I have, as occasion served, prepared such an index catalogue of all papers, memoirs, and books on these topics, and have arranged the references according to authors, alphabetically, giving for each reference volume and page. Where the title of a book or paper explains the subject of it, such title is, in general, alone given. Where a paper is quite important its title is given, and if necessary, a note, more or less brief, expressive of its contents. The works of the elder HERSCHEL on these subjects I have analyzed at considerable length, in order partly to supply the great want for an edition of his collected works. For papers of minor interest a reference to the periodical, volume and page is alone given (no title), and a note of its purport. In addition to this, and following Mr. KNOBEL, I have given a very condensed reference to the papers in each periodical consulted. In this way a person consulting the catalogue will find all the works of any author upon the general subject, with brief notes of the contents of each paper; or again, any person desiring to refer to the various papers on this subject contained in any serial publication, as the *Philosophical Transactions* for example, will find references which will save him much time. The index is practically complete to the present date, and contains perhaps 1000 references. It contains also a complete list of all published (and many unpublished) drawings of nebulæ and clusters. It is evidently of *special* value only, but knowing the labor it has already saved to me, I cannot doubt that similar works in other specialties in Astronomy, like Mr. KNOBEL's for example, will be of much use.

Mr. E. S. HOLDEN also made a communication on

THE SHADOW OF THE BALL OF SATURN PROJECTED ON THE RINGS,

describing experiments made with an accurate drawing of Saturn and its rings. The line of the projection of the shadow appeared convex towards the planet, straight or concave according to the distance of the observer from the drawing. Five persons made accordant drawings of the appearance, independently of each other.

Mr. ASAPH HALL made remarks on

A BRIGHT SPOT WHICH HAD RECENTLY BECOME VISIBLE ON THE BALL OF SATURN.

(ABSTRACT.)

Mr. HALL stated that while observing one of the satellites of Saturn on Dec. 7th, he noticed a round and well-defined spot on the ball of the planet. The spot was 2″ or 3″ in diameter, and was of a brilliant white color. It was situated a little north of the rings, and in the direction of declination was near the middle of the disk. The spot came to the centre of the disk in the equator of rotation at 6^h 18^m Washington m. t. The next day letters were sent to the astronomers of the country, and although cloudy at Washington, the spot was observed on Dec. 10th by Professor MARIA MITCHELL at Vassar College observatory; by Mr. LEWIS BOSS at Dudley observatory; by Mr. D. W. EDGECOMB at Hartford, Conn., and by the Messrs. CLARK at Cambridgeport, Mass. The spot was again observed by Mr. HALL and Mr. EASTMAN at Washington on Dec. 13th, and by Mr. A. G. CLARK at Cambridgeport; and again by Mr. HALL at Washington on Dec. 16th.

From the observations thus far made it appears that the time of Saturn's rotation, assuming that the spot has no proper motion, is

$$10^h\ 15^m.0$$

This time, as given in the modern text-books, is

$$10^h\ 29^m\ 16''.8$$

and is said to be Sir W. Herschel's last and corrected determination. On the other hand, the time of rotation published by Sir W. Herschel in the Philosophical Transactions for 1794 is

$$10^h\ 16^m\ 0^s.44$$

116TH MEETING. JANUARY 13, 1877.

The President in the Chair.

Forty-nine members and visitors present.

Mr. G. K. GILBERT made a communication on

LAKE BONNEVILLE,

the great fossil lake of Utah. He described an ancient outlet of the lake at Red Rock Pass near the town of Oxford, Idaho, by which its waters were discharged into Snake River. During and since the desiccation of the lake, the land which it covered has been tilted to the northward, in common with the region of the Laurentian lakes and the eastern and western seaboards. He further described a small movement along the line of the great fault at the western base of the Wasatch Mountains, by which the altitude of the mountains above the adjacent valley has been increased at a date far more recent than that of the ancient lake. (*A full account of his observations will appear in the publications of the U. S. Geographical and Geological Survey of the Rocky Mountain Region, in charge of Prof.* J. W. POWELL.)

Remarks were made by Mr. ANTISELL on the channel described by Mr. GILBERT as affording temporary drainage; and by Mr. ALVORD on the appropriateness of giving to this basin the name of BONNEVILLE, who was the first to make a scientific exploration of this region. Great Salt Lake for many years appeared on the maps as Lake Bonneville.

Mr. ALEXANDER G. BELL, of Boston, made a communication on

THE TELEPHONE,

which he had invented, exhibiting and describing its construction and explaining the principles on which it is operated. The sound of the human voice received on a small plate of thin Russian sheet-iron was conveyed by a telegraphic wire to a similar apparatus in another room and repeated by the vibrations of a similar iron plate. He stated that he made use of an undulatory, instead of an intermittent current, that no battery was necessary, but that the variations of intensity were produced by the vibrations of the soft iron plate, varying its distance from the poles of an electro-magnetic helix just behind it. He stated that the experiment had been successfully conducted, where the operator and hearer were 153 miles apart.

He referred also to an experiment made by Prof. HENRY many years ago, where an air played on one piano was repeated by another on the opposite side of the street, a rod of soft pine in contact with the sounding-boards of each forming the connection.

Mr. HILGARD and Mr. HENRY spoke of the value and astonishing character of Mr. Bell's discovery and invention.

Mr. BELL having spoken of the difficulty with such consonants as *p*, *t*, and *k*, Mr. MASON suggested that such an instrument, when perfected, might be used in analyzing linguistic sounds.

117TH MEETING. JANUARY 27, 1877.

The President in the Chair.

Fifty-three members and visitors present.

Mr. B. ALVORD made a communication on

A TRIGONOMETRICAL FORMULA.

Gen. T L. CLINGMAN,* of North Carolina, communicated

FACTS RELATING TO THE FALLING OF WATERSPOUTS IN NORTH CAROLINA;

speaking of the large number which had occurred in the southern and western portions of that State on the elevated plateaus or mountain sides, particularly in Jackson and Macon counties. He had visited the localities of fifty or sixty, and described particularly one in Fish-hawk Mountain, where a large hollow, 75 feet across and in the middle 15 feet deep, had been scooped out apparently by a sudden fall of a large quantity of water. The streams of the mountain were suddenly swollen to a destructive extent.

Mr. ANTISELL described a genuine waterspout, attributing the phenomenon to warm and cold currents of air in opposite directions, by friction occasioning electricity; and made further explanations of such appearances and their mode of formation.

Mr. SHELLABARGER, Mr. G. F. TALBOT, and Mr. CURTIS described waterspouts or cloudbursts which they had witnessed among mountains or hills, where the fall of water appeared to be sudden and great.

Mr. HENRY remarked on the necessity of recorded statements of facts giving fully the attending circumstances, and referred to the investigations by Espy many years ago, and to a valuable report on the subject of tornadoes in one of the Reports of the U. S. Signal Service.

Mr. J. H. C. COFFIN made the following remarks on

SUMNER'S METHOD IN NAVIGATION.

To find the longitude of a ship at sea by a chronometer, the chronometer time of one, or more, altitudes of the sun or other celestial body is noted, and reduced to *mean* time at Greenwich, or other prime meridian, by applying the chronometer correction, supposed to be known more or less accurately. Navigators usually stop here with this part of the process. It is better to go farther and find the *hour angle* at the prime meridian of the body observed. This in the case of the sun is the *apparent* time, reckoned in A. M. from the lower meridian.

The *local hour angle* is found from the latitude of the place, the declination, or polar distance, of the body, and its corrected altitude. The difference of the local hour angle from that at the prime meridian is the longitude in time.

The declinations of bodies observed at sea are known far more accurately than is requisite for navigation. The altitude is always uncertain, 2′ or more even when very carefully observed, owing to the variable refraction of the sea horizon. The latitude is the most uncertain element, as it is brought forward or carried back by the dead reckoning, from some determination made at a time several hours distant.

Here, then, is a simple case of finding the locus of an equation with two unknown quantities; and we may assume values of the latitude within reasonable limits and compute the corresponding longitudes. Unless the altitude is very great, or the latitude very uncertain, it is sufficient to assume two latitudes and find the corresponding longitudes, *i. e.* determine two points of this *line of position*.

This line is perpendicular to the direction of the object observed. If it be projected on a chart, and lines parallel to it be drawn on either side at a perpendicular distance equal to the uncertainty of the altitude, and others at a distance in longitude from these equal to the uncertainty of the chronometer correction, the position of the ship will be within the belt delineated; and this is as valuable a determination as if the latitude alone, or the longitude alone, had been found.

Again, from an altitude of the same, or another object, another line of position may be found and projected on a chart; and one of the lines being shifted for the run of the ship (including known currents) in the interval, the intersection of the two gives the ship's position both in latitude and longitude. This intersection is best determined when the azimuths for the two observations differ 90°. It may be found by computation as readily as by projection.

If the object is near the meridian, it is better to assume two or more longitudes and compute the corresponding latitudes. This, however, was not proposed by Capt. SUMNER.

This method* was first published by Capt. THOMAS H. SUMNER of Boston in 1843. His conception of the problem was purely geometrical. The sun, or any other body, at a particular instant is vertical at a place on the earth's surface, whose latitude is the declination of the body, and whose longitude is its hour angle at the prime meridian; and the body will be at the same altitude at all points of a small circle, whose pole is where the body is vertical, and whose polar radius is the complement of the altitude. An altitude of an object, when the latitude and longitude of the place of observation are unknown, simply determines the position of such a circle, or a limited portion of it depending on the accuracy with which the latitude, or the longitude, is known.

This method was strongly commended by some officers of the U S. Navy and before 1851 formed a part of the course of navigation at the Naval Academy In a few years it was very generally used in the Navies of the United States and Great Britain, and has been introduced in a more refined form in the best works on navigation; but it is not much known in the merchant services of

* A new and accurate method of finding a ship's position at sea by projection on Mercator's Chart, by Capt. THOMAS H. SUMNER; Boston, 1843.

those countries. This is attributable to the rough uncouth form in which Capt. Sumner has presented it, to the cumbersome method adopted in his computations, requiring the use of three tables, instead of one, but more, I apprehend, to his setting it forth prominently as a method of determining "the true bearing of the land," and erroneously giving the idea that the line of position is directed to, or near, the destined port. This has led to severe criticisms of the method in nautical magazines, and to its rejection by the Astronomer Royal of Great Britain, and other mathematicians. The finding the true bearing of any point of the land is entirely a distinct problem, and should not have been mixed up with this.

My first use of this method was in December, 1838, in the Gulf Stream off the coast of North Carolina. Altitudes of the sun at 9 A. M. gave a line of position nearly parallel with the coast, and thus determined the distance from the land, which at the time it was most desirable to know. Altitudes at 2 P. M. gave an intersecting line. In subsequent cruises before 1843, I made frequent use of the method, preferring it as the most convenient method of finding the latitude of a place by double altitudes, even in observations with an artificial horizon on shore, and as decidedly the best method if the local time is also to be found.

It surprised me subsequently to find that a method so naturally suggested, and which would readily occur to any mathematician who is engaged in navigation, had not been published earlier. LALANDE, however (*Astronomie*, Art. 3992 and *Abrege de Navigation*, p. 68), has given it so far as relates to finding the latitude by double altitudes.*

Lately SIR WILLIAM THOMSON has published *Tables for facilitating Sumner's Method at Sea* (London, 1876). He uses a method suggested, but rejected, by Capt. SUMNER, of finding one point of the line of position and the true bearing of the line, which differs 90° from the azimuth of the object. These tables are ingeniously devised for finding the hour angle and azimuth from an observed altitude; but it is questionable whether they facilitate the process. Intelligent navigators will prefer forms of computation to which they are accustomed: the unintelligent cannot well be trusted to use them. I think the labor of computation is greatly overrated.

* CHAUVENET's *Astronomy*, vol. i. p. 428.

I give below an example* of computation in the forms which American navigators chiefly use. The preparation of the data and Cols. 1 and 2 belong to the ordinary "*time sight*," which is employed daily in finding the longitude by a chronometer. The only *additional* work required for SUMNER's method is Col. 3 if two latitudes are used, or Col. 4 if the azimuth is to be found, together with their results at the bottom of Col. 1. As the second latitude is 20′ greater than the first, the corresponding *half sum* and *remainder* will be 10′ greater than those in Col. 1; but it is not necessary to write them down; and the corresponding logarithms in the two columns can be taken from the table at the same opening.

The ship is supposed to be in north latitude, and the altitude of the sun observed in the morning.

Greenwich ap. time by chronometer	1 2^h 54^m 5^s P. M		2 1st lat.	3 2d. lat.		4 1st lat.
Corrected alt. of ⊙	29° 6′ 25″		36° 30′	36° 50′		36° 30′
1st latitude . .	36 30 0	l. sec.	0.09482	.09670	l. sec.	0.09482
⊙'s polar distance	110 26 56	l. cosec.	0.02827	.02827	l. sec.	0.45671
Sum	176 3 21					
Half sum . . .	88 1 40	l. cos.	8.53674	.49841	l. cos.	8.53674
Half sum—alt. .	58 55 15	l. sine	9.93271	.93346	l. cos.	9.71284
			2)8.59254	.55684		2)8.80111
1st local ap. time†	10^h 28^m 43^s A. M.	l. sine ½		9.29627		
2d " "	10 32 27	l. sine ½		9.27842		
1st longitude . .	4 25 22	=	66° 20′. 5 W.			
2d "	4 21 38	=	65 24. 5 W.			
Half azimuth of ⊙	75 26				l. cos. ½	9.40056
Azimuth of ⊙ . .	N. 150 52 E.					
" of line .	N. 60 52 E.					

The line may be projected by the two positions,

1st lat. 36° 30′ N.; long. 66° 20′.5 W.;
2d " 36 50 N.; " 65 24 .5 W.;

or, by the 1st position, and the direction N. 60° 52′ E.

* *Navigation and Nautical Astronomy*, prepared for the use of the U. S. Naval Academy, p. 226, New York, 1866.

† In BOWDITCH's Table the entire hour angle is given corresponding to the l. sine of its half.

Logarithms to four places will usually give all the accuracy which observations with the sea horizon admit of.

Mr. J. W. POWELL commenced a communication on

THE PHILOSOPHY OF THE NORTH AMERICAN INDIANS.

118TH MEETING. FEBRUARY 10, 1877.

The President in the Chair.

Forty-seven members and visitors present.

The President announced the death of Rear Admiral THEODORUS BAILEY, U. S. Navy, a member of the Society.

Mr. J. S. BILLINGS made a communication on

BACTERIA AND SPONTANEOUS GENERATION.

(ABSTRACT.)

Several years ago I called the attention of the Society to this subject, and showed the results of a number of experiments, made to test the accuracy of the results reported by Dr. BASTIAN. His statements were not confirmed—but the experiments could not be considered satisfactory on account of the difficulty of preserving fermentable and putrescible fluids when free access of air is allowed.

This difficulty has been overcome, and the subject of the life history of microzymes including bacteria, is now attracting much attention, especially in relation to the causation of disease. I have recently had the opportunity of examining the results of a number of experiments made by Prof. JOSEPH LISTER, of Edinburgh, a full account of which has not yet been published, and propose to call the attention of the Society to these and to the apparatus which he uses. Mr. LISTER was a believer in the teachings of PASTEUR, that all fermentations and putrefactions are due to the presence of living organisms, and on this he based his method of excluding these organisms from wounds in order to prevent putrefaction. (His apparatus was shown.)

But in 1871 a paper was published by Dr. BURDEN SANDERSON which seemed to prove that drying bacteria killed them, and that therefore the dust of the air contained none of them living. As this would have affected Mr. LISTER's process very much, he undertook a new series of experiments. (Apparatus shown and

peculiar difficulties attending experiments with milk, turnip solution, peas, beans, and hay explained.)

From the results obtained by Mr. LISTER, and from those reported by RAY LANKESTER of London, and Prof. COHN and his assistants at Breslau, it may be considered as certain that the so-called doctrine of spontaneous generation is incorrect, and is not a permissible theory to invoke to explain the very puzzling phenomena observed in connection with the development of these minute organisms.

These organisms may be divided into two classes—those which seem to be everywhere present, and those which have specific qualities. (For 1st Class, see Note of M. MARIE DAVY, *Comptes Rendus*, Dec. 27, 1876. For 2d Class, attention is called to KOCH'S researches on splenic fever and CURTIS on diphtheria.)

The classifications of COHN, BILLROTH, and LISTER are not considered satisfactory; they deal with these minute organisms as if they formed an independent class, while really they should be considered in connection with the minute algæ and fungi. To attempt to distinguish species by the microscope or even by chemical tests, will fail. The culture test in various substrata is the only mode of solving the problem.

Mr. J. W. POWELL continued a communication on

THE PHILOSOPHY OF THE NORTH AMERICAN INDIANS,

describing peculiarities of language, theology, religion, mythology, treatment of diseases, and nursery tales.

Mr. GILL called attention to the use of the same terms, in zoology and linguistics, as generate, differentiate, and specialize, but in different senses.

Remarks were also made by Messrs. NEWCOMB, WHITE, ANTISELL, WARD, and WELLING.

119TH MEETING. FEBRUARY 24, 1877.

The President in the Chair.

Thirty-six members and visitors present.

The election of Captain WILLIAM NICHOLSON JEFFERS, U. S. Navy; Commander MONTGOMERY SICARD, U. S. Navy; ANECITO

G. MENOCAL, Civil Engineer, U. S. Navy; EDWARD CLARK, Architect of the Capitol, and MARTIN FRANCIS MORRIS, as members of the Society, was announced.

The President announced the decease within a brief period of Prof. F. B. MEEK, Gen. A. B. EATON, U. S. Army, and Rear Admiral C. H. DAVIS, U. S. Navy, members of the Society; and, on motion of Mr. PARKER, the President, Mr. MEIGS, and Mr. NEWCOMB were appointed a committee to prepare suitable commemorative resolutions respecting them and the late Rear Admiral BAILEY.

Mr. E. B. ELLIOTT made

COMMENTS ON THE TELEPHONE,

comparing the instruments and methods of Mr. GRAY and Mr. BELL.

Some discussion followed, in which Mr. TAYLOR and Mr. COFFIN participated, from which it appeared that the instrument of Mr. GRAY referred to by Mr. ELLIOTT was not that exhibited by him to the Society February 12, 1876, but one of an earlier construction, and like that of Mr. BELL so far as employing the vibrations of a thin metallic plate.

Mr. A. F. A. KING read a paper on

THE CONSERVATIVE ELEMENT IN DISEASE,

defining disease as a *tertium quid* resulting from two factors: 1st. The impressions of new environing conditions; and 2d. The responsive changes in the organism following and resulting from such external stimuli. The new formations, instead of being destructive, were designed to secure adaptation to the new environment. Modifications of the organism, thus originating, were analogous with physiological formations in the several particulars of (1) having the same design of adaptation to environment; (2) in being gradual and latent in their development; (3) in tending to follow a typical course; (4) in requiring the same condition to secure their designed completion; and (5) in being liable to attacks of acute inflammation on exposure of the body to cold during their evolution.

Organisms undergoing conservative modification were liable to fail in reaching the designed completion of adaptive change, from (1) the environing changes of condition having been exaggerated *in degree;* (2) their being inconstant, or vacillating *in kind;* or (3) multiplied in *number.*

Many pathological changes, apparently destructive, might still be conservative, our lack of knowledge not enabling us to understand how they had contributed to prolong life. Organs of lesser importance were often impaired in function and structure, to preserve the functional integrity of other organs whose office was more directly essential to life: *e.g.* the joint affection in rheumatism served to prolong life, by lessening the labor of the heart —the latter organ being saved from fatal overwork by the joint disease restraining motion—especially locomotion—on the part of the patient.

Diseases sometimes end fatally because the instinctive desires or cravings that accompany them are disregarded, or their gratification is opposed by medical opinion; when, in fact, they were the best guides to the hygienic requirements of the body. The success attending the permission to drink and bathe during fever (the *new* mode of practice) was very striking compared with the old mode of practice, which forbade, or greatly restricted, drinking, and bathing in cool water.

The subject was discussed by Messrs. DALL, WOODWARD, TAYLOR, and FARQUHAR.

Mr. G. K. GILBERT made a communication on

THE STRUCTURE OF THE HENRY MOUNTAINS

in the middle of the Colorado plateau region.

120TH MEETING. MARCH 10, 1877.

The President in the Chair.

Thirty-two members and visitors present.

Mr. S. NEWCOMB read a paper on

THE COSMOGONY;

reviewing the various speculations on the development of the solar system, especially those of Kant, La Place, and Herschel; referring also to the latter's examination of nebulæ.

A discussion followed in which Messrs. POWELL, TAYLOR, ANTISELL, NEWCOMB, E. B. ELLIOTT, MARSH, and HENRY participated.

121ST MEETING. MARCH 24, 1877.

The President in the Chair.

Fifty-six members and visitors present.

The election of Mr. MONTGOMERY MEIGS as a member of the Society was announced.

Mr. G. K. GILBERT continued his communication on

THE HENRY MOUNTAINS,

describing their geological formation, and particularly the peculiar volcanic structure in some localities, where the lava did not reach the surface, but was injected between strata and caused a bulging of those lying above.

(This paper will appear in the Reports of the U. S. Geographical and Geological Survey of the Rocky Mountain Region.)

Mr. POWELL spoke of other peculiarities of these mountains.

Mr. T. N. GILL made a communication on

THE RELATIONS AND SEQUENCES OF THE FAMILY CENTRARCHOIDES.

Remarks were made by Mr. HILGARD and Prof. D. S. JORDAN of Butler University, Indianapolis.

Mr. MARCUS BAKER read the following paper on

THE HISTORY OF MALFATTI'S PROBLEM.

In the Transactions of the Royal Society of Edinburgh, Vol. 24, 1864–67, pp. 127–138, Mr. H. F. Talbot inserts a memoir entitled "Recent Researches on Malfatti's Problem." After a brief introduction, in which the problem is given with some reasons why it has proved so interesting, he gives a history of the problem; but there are some important papers upon the subject which Mr. Talbot has overlooked, and in consequence his history contains some errors. The object of the present paper is to call attention to the papers overlooked by Mr. Talbot, and to rewrite the history of the problem.

The problem to inscribe in a triangle three circles, each touching the other two, and also two sides of the triangle, was first solved by an Italian geometer, Mr. John Francis Malfatti, in 1803, and from his solution being the first that has been given, the problem is now called Malfatti's Problem. His solution is given in the tenth volume of the Memoirs of the Italian Society of Sciences. The proof which Mr. Malfatti gave of the correctness of his construction is, according to Mr. Talbot, the following: He deduces from his construction a value of one of the radii of the required circles, and also deduces a value of the same radius resulting from the conditions of the problem; and these results being found to be identical, he concludes the correctness of his construction.

The expressions found by Mr. Malfatti, are the following:—

In a triangle $A\ B\ C$, whose sides are a, b and c, call the radius of the inscribed circle r, the radii of the three circles touching each other r_a, r_b and r_c; r_a touching b and c, r_b touching c and a, and r_c touching a and b; also call the distances from the centre of the inscribed circle to the three vertices A, B and C, α, β and γ respectively. Then we have as the expressions found by Malfatti

$$2r_a = \frac{r}{s-a}\left\{s-r+\alpha-\beta-\gamma\right\}$$

$$2r_b = \frac{r}{s-b}\left\{s-r-\alpha+\beta-\gamma\right\}$$

$$2r_c = \frac{r}{s-c}\left\{s-r-\alpha-\beta+\gamma\right\}$$

The next writer upon the problem appears to have been Gergonne, who proposed the problem in the first volume of his

Annales, in 1810. It was not answered by his correspondents, and accordingly he takes up the problem himself in the second volume, and gives an analytic investigation; but no simple geometric construction results from his analysis.

Dr. A. L. Crelle, of Berlin, published a trigonometric solution of the problem in a collection of mathematical problems published in 1821, in Berlin, and in 1827, Prof. Lehmus, also of Berlin, published a trigonometric solution of the problem in the second volume of a course in pure and applied mathematics.

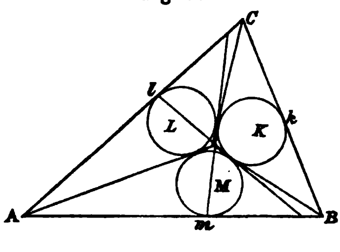
Up to this time, then, viz., 1827, no geometrical proof of the construction of the problem had appeared.

The next writer upon Malfatti's problem was Prof. Steiner, of Berlin. In the first volume of Crelle's Journal he has a paper entitled "Some Geometric Considerations," which is dated at Berlin, in March, 1826. In this paper he says, that "about three years since the author of this paper became interested, accidentally, in the consideration of the problems: *First*, to describe a circle tangent to three given circles; *second*, in Malfatti's Problem; *third*, in Theorem, XV, Book IV of the *Collect. Mathemat.* of Pappus; and *fourth*, in several porisms and the purely geometric considerations of curves and areas of the second degree. Pappus's theorem he was familiar with, but without proof; so also Malfatti's Problem; of the first, however, he was acquainted with Vieta's geometrical solution."

Further on he adds: "The effort of the author was, in the solution of the different problems with reference to the tangencies of circles, to find the underlying general principles by which they were connected."

Then three paragraphs follow in which the fundamental principles of radical axes and centres of similitude are developed, after which, "to show the fruitfulness of the preceding theory by a suitable example," this elegant solution of Malfatti's Problem is given: Bisect the angles of the triangle, and in the three partial triangles so formed inscribe circles. From the point of tangency m of the circle M (Fig. 1), with the side

Fig. 1.



A B, draw a tangent to the circle L (which will also be tangent to the circle K), and from the point of tangency l of the circle L, with the side A C, draw a tangent to the circle M (which will also be tangent to the circle K); in the quadrilateral thus formed, A l O m* (which is a circumscriptible one), inscribe a circle, and it is one of the circles required; in a similar manner the remaining circles are found.

The proof of this construction, as also the extension of it which follows, is omitted, and the expression "jedoch ohne beweis" leaves us in doubt whether he merely omitted the proof, or whether he had not yet found a proof to omit.

It is proper to add that Steiner does not stop with merely giving a solution of the problem, but by means of the principles of Radical Axes, Centres of Similitude, etc., which he had developed in the preceding paragraphs, he goes further and solves this more general problem. *Three circles in a plane are given in magnitude and position, describe three other circles each touching the other two and two of the given circles.* And even this extension is still further extended to the case of three circles lying not in a plane but upon the surface of a sphere.

After giving the solution of Malfatti's Problem without the extension, he adds that the problem by no means admits of merely a single solution, but that at least thirty-two different solutions of the problem without the extension seem to be possible, all similar to the foregoing.

These statements are given without proof, and, so far as known to the writer, no geometric proof of this solution *with the extensions* of the problem has been given. It is true that one of the thirty-two solutions has been given by several persons, but nowhere apparently by the methods used by Steiner, unless perchance that by Andrew S. Hart, in Vol. I of the Quarterly Journal of Pure and Applied Mathematics, be such.

In the following year (May 2, 1827) Prof. Paucker, of Mitau, presented to the Imperial Academy of Sciences of St. Petersburg, a memoir on a question relative to the tangencies of circles, and the memoir, which covers 84 4to. pages, is almost exclusively devoted to Malfatti's Problem. This memoir seems to have been generally overlooked; at least no reference to it has been

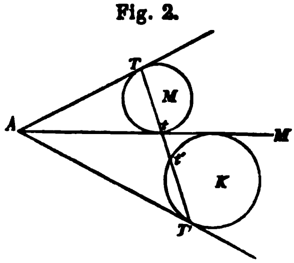
* O is the centre of the inscribed circle, not placed upon the figure.

anywhere found, and it is the failure by Mr. Talbot to examine this memoir that has led him into error in his history.

Professor Paucker first analyzes the problem, and then gives a solution. The solution which he gives is almost identical with that given by Steiner. It differs, however, in this: after having inscribed circles in the three partial triangles (Fig. 1) the centres of the three circles are joined, two and two, and then from m a line is drawn through the intersection of K L with O C, and he then shows that this line will be tangent to the two circles K and L; whereas Steiner draws a line from m tangent to the circle L, and he says it will be tangent to the circle K also. This is a slight difference in getting at the same result, but combined with the dates of their papers, which differ by a little more than a year, and with the manner in which Prof. Paucker proves his construction, and also combined with the fact that he makes no allusion to the work of Steiner, while he gives due credit to Malfatti, Gergonne, Crelle, Lehmus, and Tédenat, seems to indicate that he had obtained his results independently of Steiner.

Prof. Paucker demonstrates the correctness of his construction by Euclidian methods, making no use of the Modern Geometry, so called, and goes into the details with much carefulness. He begins by demonstrating some Lemmas, and in this respect has been followed by all subsequent writers. His first Lemma is as follows: If an angle A is bisected by A M (Fig. 2) and two circles are drawn at pleasure, one tangent to A M and A T, and the other tangent to A M and A T′, and the points of tangency T and T′ are joined, then the cords T t and T′ t' are equal. This he readily proves by similar triangles.

Fig. 2.



In paragraph 13 of his memoir he demonstrates that if tangents are drawn (Fig. 2) from T to the circle K, and from T′ to the circle M, these tangents are equal.

These two propositions are also proved by Mr. Hart in the first volume of the Quarterly Journal, without reference to whether it had previously been done.

Prof. Paucker gives, in addition to the solution of the problem before mentioned, a second solution entirely independent of the auxiliary circles, and which results immediately from a theorem due to Mons. Pierre Tédenat, and which theorem is proven in his memoir. He also gives the trigonometric solution as modified from that of Crelle and Lehmus, and ends with several miscellaneous theorems on tangencies. The memoir is very complete from the standpoint of the old geometry, but the author does not appear to have had so broad a view as Steiner, and nowhere goes into the question of how many solutions are possible as Steiner has done. Nevertheless his paper is one of the most important contributions to the subject, and one which has nowhere been found mentioned by any writer upon the subject that has been consulted.

In 1833, Prof. Zornow, of Königsberg, published in Crelle's Journal, Vol. X, an algebraic demonstration of Steiner's construction, and the next year Prof. Plücker, of Bonn, published in the same journal a demonstration of the same, partially geometric, but it is only completed by the aid of analysis. His memoir bears date Oct. 1831, and "he was, therefore," says Mr. Talbot, "the first who succeeded in demonstrating Steiner's theorem." It would seem, however, that Prof. Paucker, whose memoir was read before the St. Petersburg Academy, May 2, 1827, is entitled to the credit, as he seems both to have discovered and demonstrated Steiner's theorem.

In Hymer's Trigonometry, published in 1842, a very good trigonometrical solution of the problem may be found, the results obtained being the same as those found by Mr. E. B. Seitz, in the Analyst, Vol. II, 1875, pp. 74–76.

The next paper of importance seems to have been one by Prof. Adams, of Winterthur, who published a complete algebraical investigation of the subject in 1845, in a quarto pamphlet of 26 pages. The pamphlet does not appear to be accessible in any of the libraries of this city, and the contents of the pamphlet can only be judged from a brief review of it found in Nouvelles Annales de Mathématiques, Vol. VIII, p. 62. It is there indicated that the pamphlet consists principally of twelve Lemmas algebraically proved.

In 1852, Prof. Schellbach, of Berlin, published in Crelle's Journal, Vol. XLV, a new solution of Malfatti's Problem, accom-

panied with a trigonometric proof, and subsequently the same construction was extended to spherical triangles.

This makes three entirely different constructions, viz., the one which Prof. Paucker says results immediately from a theorem of Tédenat's, and which perchance might be called Tédenat's construction; second, Steiner's; and third, Schellbach's. A consultation of all the authors referred to would doubtless reveal others.

A paper by Mr. Andrew S. Hart has been already referred to. It is important, though brief, as apparently coming nearer in its mode of thought to the reasoning that led Steiner to the discovery of the solution than any paper met with.

Prof. Cayley has also written upon the problem. He has a note on Schellbach's solution in the first volume of the Quarterly Journal, and in the Transactions of the Royal Society, Vol. LXX, he has a memoir entitled "Analytic Researches connected with Steiner's extension of Malfatti's Problem."

Lastly, we come to the memoir of Mr. Talbot, mentioned at the outset. He says of Malfatti's Problem that "although it is a question of elementary geometry which can be solved by a simple and elegant geometrical construction, yet no *geometrical* proof has ever been given, as far as I am aware, of the truth of this construction. * * * I now offer," he says, "to the Royal Society a purely geometrical solution of the problem; and, for the sake of clearness, I have divided it into several parts, which I have called Lemmas." Then follow the Lemmas, of which there are 11, "in the first 9 of which," he says, "I chiefly follow Plücker." * * * But Lemmas 10 and 11 are original; at least he believed them to be so.

Lemma 10 is the general case of the first Lemma of Prof. Paucker, already cited, *i. e.*, if in Fig. 2, A M is not the bisector of the angle A then the chords

$$\frac{\mathrm{T}t}{\mathrm{T}'t'} = \frac{tg\,\frac{1}{2}\,\mathrm{M\,A\,T}}{tg\,\frac{1}{2}\,\mathrm{M\,A\,T}'}$$

and this general case is proved by Mr. Hart.

Mr. Talbot's 11th Lemma is identical with the 13th paragraph of Paucker's memoir above cited.

In conclusion it may be remarked that in none of the works consulted in preparing this sketch has there been found a treatment of the problem in the general way in which Steiner handled

it; and it seems that a complete analysis of the various cases and solutions for plane and spherical triangles, and for spheres from a purely geometric point of view, is still wanted.

A list is appended of references to all literature that has been found containing anything pertaining to Malfatti's problem.

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MALFATTI (Giovanni Francesco). Memoria sopra un problema stereotomico [1802]. Memorie di Matematica e di Fisica della Società Italiana delle Scienze residente in Modena. Vol. x., part 1, 1803, pp. 235–244.

MARTIN (Artemas). Proof of Steiner's solution. Lady's and Gentleman's Diary for 1870, pp. 89–90.

PAUCKER (Magnus Georg von). Memoire sur une question de géométrie relative aux tactions des cercles.
Mémoires présentés a l'Academie Impériale des Sciences de St. Petersbourg par divers Savans. Vol. i., 1831, pp. 503–586.

PLUCKER (Julius). Das Malfattische Problem.
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* Lechmütz and Lehmus I suspect to be the same, but have not been able to verify the fact.

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Transactions of the Royal Society of Edinburgh. Vol. xxiv., 1867, part 1, pp. 127–138.

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VAN SWINDEN (Jan Hendrik). Elemente der Geometrie aus dem Holländischen übersetzt und vermehrt, von C. F. A. Jacobi. 8vo. Jena, 1834, p. 256, No. 675.

WITTSTEIN (Armin).* Geschichte des Malfattischen Problems. 4to. 39 pp., 4 pl. München, 1871.

ZORER (——). Programm des Gymnasiums zu Ellwangen. Tübingen, 1870.

* Since the reading of this paper Prof. E. S. Holden, of the U.S. Naval Observatory, has called my attention to the history by Wittstein. While this is going through the press I have received a copy of Wittstein's history of the problem, and from it I extract the titles of several papers mentioned in it, which papers I have never seen, and incorporate them in the foregoing list. It may be observed that Mr. Wittstein has, as well as Mr. Talbot, overlooked Prof. Paucker's memoir in making up his history.

ZORNOW (Anton Robert). Démonstration de la solution du problème de Malfatti, donnée par Mr. Steiner, p. 178, du tome i., cah. 2.
Journal für die reine und angewandte Mathematik, von A. L. Crelle. Vol. x., 1833, pp. 300–302.
Nouvelles Annales de Math. Vol. vi., 1847, pp. 346–350.

Mr. HILGARD called attention to a paper on the general problem of tangencies in the Smithsonian Contributions to Knowledge by Mr. ALVORD.

122D MEETING. APRIL 7, 1877.

The President in the Chair.

Fifty-one members and visitors present.

Mr. C. ABBE, in the absence of Mr. PARKER, Chairman, read the report of the Committee on

THE METEOR OF DECEMBER 24, 1873.

(This report will appear hereafter.)

Remarks were made by Messrs. HILGARD, HARKNESS, and COFFIN, mainly on the difficulty in obtaining trustworthy observations on meteors, sufficient for determining their apparent paths and altitudes.

Mr. B. F. GREENE made a communication on

THE NAVY COMPASS,

exhibiting and contrasting the Liquid Compass now provided, and such as were in use only a few years ago, and describing the great improvements which have been introduced, greatly increasing precision, delicacy, and stability.

Remarks were made by Mr. HARKNESS, mainly on the device of Mr. RITCHIE for elevating the compasses on board the monitors, and Messrs. HILGARD, COFFIN, F. M. GREEN, ABBE, and ALVORD.

123D MEETING. APRIL 21, 1877.

Vice-President TAYLOR in the Chair.

Mr. A. F. A. KING, in continuance of his former paper (read February 24, 1877), presented remarks showing

THE CONSERVATIVE INFLUENCE OF DISEASE AS ILLUSTRATED IN THE PHENOMENA OF PULMONARY PHTHISIS.

(ABSTRACT.)

Speaking generally, and in conformity with the views previously presented, he concluded: 1st. That the *primary cause* of conservative organic change, was change of environment which necessitated modification or function. 2d. To *prevent* such changes taking place, the natural environment must be continued. 3d. To *arrest* the progress of commencing modification, the new environments must be removed—the old ones restored. 4th. When this last cannot be done, the structural modifications resulting from the new environments must be conducted to their designed completion.

Successfully to carry out this course a knowledge of the adaptive relationship between the new structures and new environments must be acquired.

To this end the case selected for study must be a typical and completed one, characterized chiefly by the qualities of *latency* and *chronicity*—comparative absence of symptoms and slowness of growth. The points to be ascertained are: 1st. How has the modified organ been made to differ, in function, from the physiological *un*modified organ? 2d. What environing conditions have rendered the functional modification necessary or desirable for the prolongation of life?

To answer these inquiries, animals of different species and variety may be studied with a view to discover individuals whose organs, in their physiological state, resemble those of the modified human organism. The environments natural to such animals would necessarily resemble those producing resembling modification in man; hence the *cause* of the acquired modification in man would be thus reached.

Again, by studying individuals having structural peculiarities exactly *opposite* to those observed in the human specimen, we learn by *contrast* what we before learned by *resemblance*.

In applying this method of study to pulmonary phthisis, after selecting a "typical" completed case of the disease for special examination, we find the structural changes to be (chiefly): dilatation of the bronchi with chronic peribronchitis; thickening and adhesion of the pleura; obliteration of air-vesicles and capillary bloodvessels in the lung, the normal lung tissue being substituted by a hyperplasic growth of connective, or fibrous, or even fibro-

cartilaginous tissue. These changes are commonly most marked in the *left* lung.

On examination, as before directed, we find that animals whose *normal* respiratory system more or less resembles, in structure and function, the respiratory apparatus of the consumptive patient, are to be found among the different tribes of serpents, frogs, toads, tortoises, turtles, etc. The natural environments of these animals were such as to demand of their respiratory organs only a limited amount of function as compared with those of man; hence it was inferred that partially similar environments must have impressed the human individual in whom the chronic changes of structure, just noticed, were developed, and which he therefore designated *Fibrotic Atrophy of the Respiratory Apparatus.*

The conclusion thus reached he considered was sustained by actual observation in practice. Thus *muscular indolence*, especially such as is inseparable from a sedentary life and indoor occupations or confinement in prisons; and prolonged silence or *vocal indolence*, together with persistent *mental depression*, were universally acknowled first causes of phthisis; in all of them the demand for respiratory function was reduced, and the consequent modification of the respiratory organs was a conservative attempt to adapt the individual to his unnatural surroundings. Opposite environments, viz., such as necessitated active locomotive exercise, vocal exercise, and promoted hilarity and mental exhilaration, were admitted prophylactics of consumption. Hence orators, pedestrians, and pugilists (the latter, however, only prior to their "retirement from the ring") were comparatively exempt from the disease.

Active respiratory function also promoted activity of the portal circulation, and consequently a good digestion, hence the theory that the disease was due to imperfect assimilation of food (a theory ably supported by Sir James Clark, Dr. Wilson Philip, Prof. J. H. Bennett, and others) was admitted only with the proviso that the impaired assimilation arose from impaired portal circulation consequent upon the restriction of the motions of the diaphragm and abdominal walls that were inseparable from a restriction of the respiration.

The fibrotic atrophy of the respiratory system was conservative, considering the environments under the impression of which it arose, in protecting the individual from fatal hyperoxidation of the blood—a condition actually existing during the first stage of phthisis, as proved by the arguments and experiments of Beddoes, Liebig, Ancell, Simon, Rokitansky, and others.

Mr. KING concluded his paper with the remark that the views he had presented were in conformity with the principles of Darwinian evolution. Man had attained his present nobility of development by successive and gradual increments of functional

exercise, extending through many centuries. Coincident with the acquired powers of speech, musical expression, and bipedal locomotion in the erect posture, the respiratory organs had attained a superior development. But such a high degree of development could not be maintained without corresponding high degrees of function; hence functional indolence tended to produce a retrogressive evolution towards lower forms of animals from which man had been evolved. Consumption, therefore, was an attempted adaptive modification of the breathing organs, in the direction of atrophy, consequent upon the failure of the individual to perform those exercises by which the highly elaborated respiratory system would be called upon for the full capacity of its functional play.

Remarks were made by Messrs. WOODWARD and GILL.

Mr. J. J. WOODWARD read the following paper on

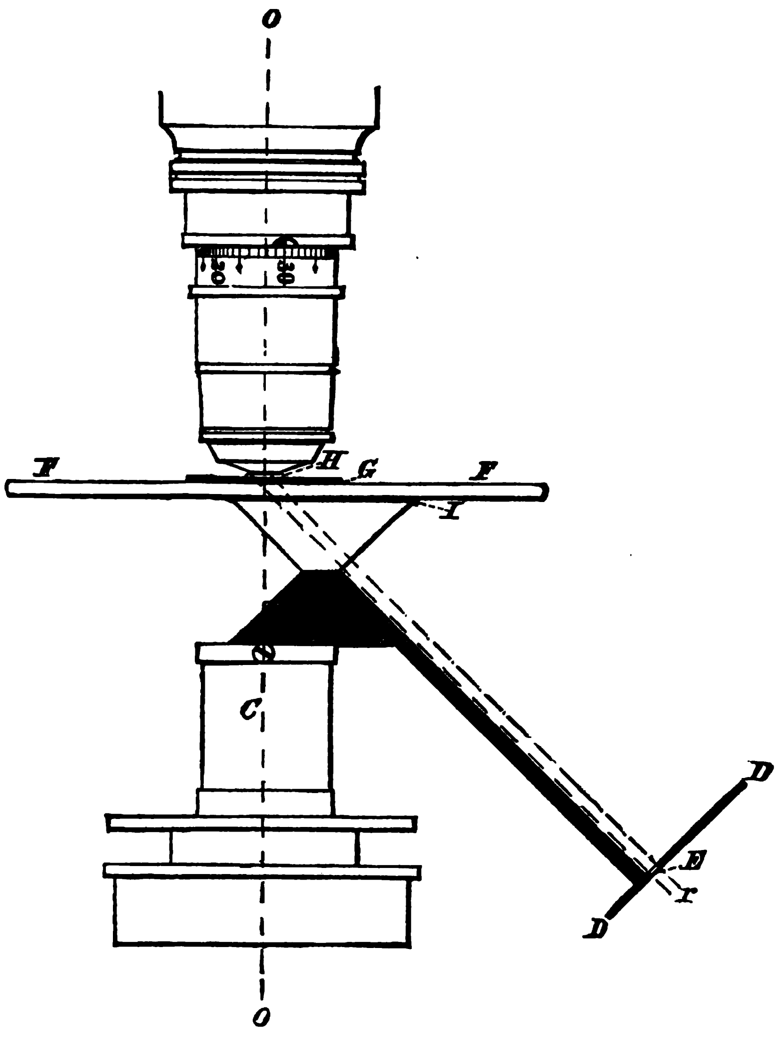
A SIMPLE DEVICE FOR THE ILLUMINATION OF BALSAM-MOUNTED OBJECTS FOR EXAMINATION WITH CERTAIN IMMERSION OBJECTIVES WHOSE "BALSAM ANGLE" IS 90° OR UPWARDS.

Certain immersion objectives are so constructed that they are capable of admitting rays which enter the front lens at a greater angle with the optical axis than the limit for dry objectives. That this is not only theoretically possible, but that such objectives have been successfully constructed, was several years since demonstrated in the Monthly Microscopical Journal, both by Mr. Keith and myself,* notwithstanding which the contrary has often since been energetically asserted by writers in the same journal.

Meanwhile, immersion lenses possessed of the excessive angle in dispute, continue to be put into the market by more than one maker, and perhaps some of the purchasers will be interested in a simple device which I have used for some time with such objectives to illuminate test-objects mounted in balsam. This device consists merely of a right-angled prism of crown glass mounted beneath the stage in such a manner that its long side can be connected by oil of cloves, or some similar fluid, with the slide on which the object is mounted. The details of the plan will be understood from the accompanying diagram, in which the glass prism is seen in section just beneath the object-slide *F, F*.

* June, 1873, p. 268; November, 1873, p. 210; March, 1874, p. 119; September, 1874, p. 124.

Just below it is another right-angled prism, of the same dimensions, made of brass; the section of this prism is indicated by dark shading in the diagram. The right angles of both prisms



are truncated, and the facets are cemented together in such a manner that the long sides of the prisms are parallel. The brass prism slips transversely in a groove in the top of a holder, *C*, which is fitted into the substage of the microscope. *D D* is a

blackened brass screen held in position by two brass arms, one of which is shown in the figure. This screen is parallel to the adjacent face of the glass prism, and has in it a small circular aperture, *E*, about the size of a large pinhole. The side of the glass prism next the screen is covered with black paper in which is a corresponding pinhole. The two pinholes are so placed that a beam of parallel white sunlight (*r*) passing through both will be perpendicular to the side of the glass prism on which it impinges.

To use this apparatus, it is adjusted in the sub-stage of the microscope, a drop of oil of cloves is placed on the upper face of the prism, the glass slide *F F*, on which the object is mounted in Canada balsam under the usual thin cover, *G*, is placed on the stage, and the sub-stage is racked up until the drop of oil of cloves is spread out into a thin layer, *I*.

The object being thus arranged, it is evident that if a beam of parallel solar rays (white sunlight), reflected from a plane mirror, be thrown through the two apertures upon the face of the prism, being perpendicular to that face, it will enter and pass through without refraction until it reaches the upper surface of the thin glass cover, *G*. The parallel rays impinge upon this surface, as is evident from the construction at an angle of 45° with the optical axis, *O O*. If, now, the medium next above the thin cover, *G*, be air, this obliquity will be greater than the critical angle, and total reflection of the rays will take place. If, however, the medium next above the thin cover be water, the obliquity will *not* be greater than the critical angle. Refraction having taken place, the rays will enter the water, *H*, and if an immersion lens of sufficient angle of aperture be focussed upon the objects mounted beneath the cover, *G*, these rays not merely enter the front of the objective, but will form a well-defined image of the object on a brightly illuminated field, which will be visible through the eye-piece of the instrument in the usual way. Of course it is evident from the diagram that with no dry objective, or any immersion objective of less than 90° balsam angle, can anything whatever of balsam-mounted objects* thus illuminated be seen.

* The apparatus can be used, of course, to secure black-ground illumination of suitable dry objects if they are mounted on the slide instead of the cover, as is usual.

Immersion objectives may be divided according to their behavior with this apparatus into three classes: 1st. Those with which, since they do not have sufficient angle of aperture to admit the illuminating pencil, nothing can be seen, precisely as in the case of dry objectives. 2d. Those which have sufficient angle of aperture to admit rays of this obliquity, but are incapable of bringing them to an image-forming focus; with these the field appears well illuminated, but the objects are not well defined. 3d. Those which not only admit rays of this obliquity, but form well-defined images with them. To this class belong not merely immersion objectives with the so-called duplex fronts, but others; and I may add not merely objectives of American make, but some constructed by a well-known English house. As might be expected, the quality of the image formed by the direct rays of the sun thrown through a pinhole at this excessive obliquity, varies very greatly in different cases. I will state, however, that I have thus far found at least seven objectives, some of English, others of American make, which define sufficiently well under these circumstances to resolve *Amphipleura pellucida* mounted in Canada balsam. With the objectives which performed best, the field was of exceeding whiteness and brilliancy, but by no means dazzling, the frustule undistorted and the striæ clean and black on the white ground, very little color-aberration being perceived. With other objectives there was more or less color-aberration and distortion, both which faults were in one or two cases very conspicuous, although in the part of the frustule most sharply focussed upon, the striæ were handsomely brought out. The objectives with which I thus succeeded, ranged all the way from a $\frac{1}{4}$th to $\frac{1}{18}$th immersion. I will add, that the objectives which resolved *Amphipleura pellucida* under these trying circumstances, when used in the ordinary way with this or other test objects, displayed an exquisite perfection of definition, which it would be hopeless to expect to attain with objectives of less angular aperture.

As it is no part of my purpose in this communication to provoke ill-tempered discussion of the merits of individual makers, I will not append a list of the results obtained with the various immersion objectives I have tried in this way. The apparatus can be constructed for a few shillings, and those who take the

trouble to use it will soon see to which of the three classes any particular objective they may test belongs.

Mr. WOODWARD also made remarks on

THE RULINGS ON GLASS BY MR. ROGERS, OF CAMBRIDGE.

(ABSTRACT.)

This communication was a preliminary one. Mr. WOODWARD showed photographs, which he had made, of two of Mr Rogers, rulings magnified about 1050 diameters, and compared them with rulings by Powell & Lealand, Nachet and Nobert. The object of the photographs was to show the quality of the lines. Those of Mr. Rogers appeared to have been ruled with a faulty point, and were each accompanied on one side by a series of faint parallel lines. That these were not due to diffraction was demonstrated by revolving the ruling (on the stage of the microscope) while the illuminating pencil remained stationary. The faint parallel lines revolved with the ruling.

The rulings of Nobert were superior in the quality of the lines to any of the others. The reporter expected to examine more rulings by Mr. Rogers and others, and would make hereafter a fuller communication.

124TH MEETING. MAY 5, 1877.

The President in the Chair.

Thirty-eight members and visitors present.

The General Committee reported the following resolutions respecting the late Dr. B. F. CRAIG, which, after appropriate remarks by Messrs. HENRY, PARKER, E. B. ELLIOTT, HILGARD, and WOODWARD, were adopted.

WHEREAS the members of this Society have learned with sincere grief of the death of one of its original members, Dr. BENJAMIN FANEUIL CRAIG, at the age of 49, at his residence in Washington, on April 10th, 1877, after a very protracted and painful illness, which he endured with uncomplaining fortitude; therefore be it

Resolved, That in considering the death of Dr. CRAIG, this Society deplores the loss of a member who was one of its founders, as well as one of the original members of the scientific

club to which it owes its origin, a chemist and physicist with unusually comprehensive knowledge, an exact experimenter, a man of absolute probity and uprightness of character, and of great benevolence of disposition. It is greatly to be regretted that a mental organization of so high an order, combined with such admirable personal qualities, was associated with a feeble physical frame, that wore out when it should have been in its prime.

Resolved, That the members of the Society tender to the mother and brothers and sisters of the deceased the assurance of their deep sympathy and condolence, and that the Secretaries be requested to transmit to them a copy of these resolutions, which shall also be entered upon the records of the Society.

Mr. G. K. GILBERT made a communication on

A SPECIAL METHOD OF BAROMETRIC HYPSOMETRY.

(ABSTRACT.)

In the determination of altitudes by means of the barometer there are two chief sources of error. Where, as is the usual case, it is sought to ascertain how much a barometer read at what is called the *new station* is higher than another barometer read simultaneously at what is called the *base station*, there are two important factors of which it is practically difficult to take account. First, since the air is not in a state of equilibrium, it cannot be known that a barometer suspended in the air above the base station, and at the level of the new station, would record the same pressure that is observed at the new station; and the discrepancy between the barometer at the new station and the barometer in air, measures a factor of the problem, which may be called the *error of gradient*. Second, since the temperature of the air is not constant, and since the air contains unequal proportions of moisture at different times, there is a variation in the density of the air independent of its pressure; and the difference between the actual density of the air and a certain assumed density, regarded as normal, measures a factor of the problem, which may be called the *error of density*.

The error of gradient will not here be considered.

The error of density is usually sought to be measured by observing the temperature and moisture of the air at the base and new stations, but this method is confessedly one of great inaccuracy. The observations are necessarily made at the surface of the ground, where the changes of both temperature and moisture are so rapid and great that they are not at all comparable with

the changes which affect the column of air to be measured; namely, that column which rests upon the base station and extends upward to the level of the new station. To obviate this difficulty, at least two devices have been resorted to. First, by establishing bases at several different altitudes and referring each new station to that base which lies most nearly in its own level, the error, which is proportional to the desired difference of altitude, has been reduced in amount. Second, by using mean temperatures and mean humidities, in place of those recorded at the hours of observation, and thus ignoring the small but actual change of temperature that the column each day undergoes, it has been found that better results have been obtained than by the direct use of the coincident observations for density.

In the method which I here propose, no observations are made of the two factors of temperature and moisture; but their resultant, the density, or more strictly a co-efficient of density, is determined directly. Within or near the district which contains the new station, I propose to establish two base stations, which shall be separated as little as possible horizontally, and as much as possible vertically; I will then measure simultaneously the pressure at each of the bases and at a new station; and then, assuming that the co-efficient of density is unity, will estimate by the aid of the usual barometric tables the difference of altitude between the two bases, and the difference of altitude between the new station and one of the bases. Then, by dividing the estimated difference of altitude of the two bases by their actual difference of altitude, previously and independently determined, I will obtain the co-efficient of density, which, being applied to the estimated difference of altitude between the new station and one base, will give the required difference of altitude.

This method is especially applicable to a mountain region, in which the error of density is usually more important than the error of gradients; and it can be most economically employed where the number of new stations referable to a single pair of bases is large.

Mr. Thomas Antisell read a paper, by Prof. C. E. Munroe, of the U. S. Naval Academy, on

THE ESTIMATION OF MANGANESE AS PYROPHOSPHATE.

(This paper was offered in January, and is printed in the American Chemist of February, 1877.)

Mr. Antisell also made a communication, entitled by him

CHEMICAL REMARKS ON TERRESTRIAL GEOGONY.

125TH MEETING. MAY 19, 1877.

Vice-President WELLING in the Chair.

Thirty-three members and visitors present.

Mr. S. C. BUSEY read a paper on

THE INFLUENCE OF THE CARDIAC AND RESPIRATORY MOVEMENTS UPON THE MOTION OF THE LYMPH.

(ABSTRACT.)

He admitted the influence of absorption taking place at the periphery, and of the excess of the arterial over the venous blood pressure, as factors in promoting the movement of the chyle and lymph, but denied their predominance; and insisted that the anatomical arrangement of the lymphatic system, and its connection with the venous system near the confluence of three large trunks near the heart, antagonized the theories of Flint and Ricklinghausen. Special attention was directed to the number and arrangement of the valves in the tubular channel, which not only prevented regurgitation, but enhanced the influence of the forces derived from position, pressure of contiguous parts, respiration, and venous current in the large vessels near the heart; and claimed that the entrance of the thoracic duct into the subclavian vein near the junction of the internal jugular was an illustration of the principle of Venturi, whereby the more rapid current of the blood necessarily contributed to the slower current in the thoracic duct. Then followed the citation of cases of obstructive and regurgitant heart diseases, illustrating the effects of impediments to the circulation upon the movements of the chyle and lymph, and exhibiting the pathological phenomena attributable to slowing of the chyle and lymph current. The paper concluded with a description of the effects of irregular and interrupted respiratory movements upon the motion of the chyle and lymph, and the importance of preserving the normal pulse-respiration ratio in the management of pulmonary diseases.

(*This paper will appear in full in the September number*, 1877, *of the N. O. Med. and Surg. Journ.*)

Mr. ANTISELL continued his remarks on

TERRESTRIAL COSMOGONY.

Mr. JOHN C. RILEY and Mr. HENRY MARTYN PAUL were elected, by the General Committee, members of the Society.

126TH MEETING. JUNE 2, 1877.

The PRESIDENT in the Chair.

Thirty-one members and visitors present.

Mr. B. F. GREENE made a communication on

AN ADJUSTABLE BINNACLE FOR THE CORRECTION OF A SHIP'S COMPASS.

Mr. ANTISELL continued his remarks on

TERRESTRIAL GEOGONY.

127TH MEETING. JUNE 16, 1877.

Vice-President WELLING in the Chair.

Twenty-one members and visitors present.

Mr. ANTISELL, by request, continued his

CHEMICAL REMARKS ON TERRESTRIAL GEOGONY,

describing the successive processes by which the chemical elements of the original nebulous matter may have combined in forming the present earth.

The formation of the earth was further discussed by Messrs. POWELL, WARD, and DOOLITTLE.

128TH MEETING. OCTOBER 13, 1877.

Vice-President HILGARD in the Chair.

Twenty-seven members and visitors present.

The minutes of the last meeting were read and adopted.

Mr. E. B. ELLIOTT exhibited a diagram, which he had devised,

"showing for each year from 1760 to 1876, both inclusive, the relative value of gold to silver in open market; also, the average of these rates by periods of five years each; and compared with the standard mint ratio of the United States; also with that of the Latin Union."

He also made a communication on

OPTIONAL MONETARY STANDARDS,

referring to duplicate and triplicate standards, each alike authorized as legal tenders, and either of which may be used at the option of debtors.

Mr. THEODORE GILL made a communication on

THE MORPHOLOGY OF THE ANTLERS OF THE CERVIDÆ.

Incited by a recently published article by Prof. GARROD, of London, on some points in the anatomy of the Ruminants (*Proc. Zool. Soc. London*, 1877, pp. 1–18), the author had lately re-investigated the mode of growth of the antlers of the deer. The results finally reached may be summarized in the following definition of antlers:—

Antlers are horn-like appendages of frontal processes, peculiar to the deer, developed periodically and concomitantly with the sexual organs, chiefly in the males, *either as simple spikes or with a tendency to bifurcation, especially (but not exclusively) in the direction of the varying greatest or axial growth.*

The modifications of the antlers and their contour in the various forms of the family are chiefly dependent on and determined by the diverse exhibitions of this tendency, and examples of several kinds are furnished by the genera *Pudua*, *Cervus*, *Elaphurus*, and *Cariacus*.

The branches into which the antlers successively divide in those deer distinguishable by the complexity of those appendages may be homologized with each other, and in the interests of a uniform system of terminology the author proposed the following names applicable to the chief subdivisions: (1) The simple spikes of the first year and their after-growths were designated *protoceres*, (2) the anterior offshoots of the second year *deuteroceres*, and the succeeding (3) third, (4) fourth, and (5) fifth anterior offshoots,

respectively (3) *tritoceres*, (4) *tetartoceres*, and (5) *pemptoceres*. The antlers of *Cervus*, *Elaphurus*, and *Cariacus* were defined as follows:—

In *Cervus*, the antlers have the main axis continuous in the *protoceres* and incurved backwards, the *deuteroceres* are procurrent and developed as "brow-antlers," and the *tritoceres*, the *tetartoceres*, and the *pemptoceres* successively bifurcate with the *protoceres*.

In *Elaphurus*, the antlers have the main axis continued into the *deuteroceres*, which are supra-current and bifurcate into an anterior larger (and often subdivided) and a posterior smaller prong, the *protoceres* are deflected backwards, and the *tritoceres* are rudimentary or absent.

In *Cariacus*, the antlers have the main axis subspirally excurrent into the *tritoceres* which generally bifurcate anteriorly, the *protoceres* are abruptly supra-current from the tritoceres, the *deuteroceres* arise from the inner surface of the protoceres near their bases, and *tetartoceres* are in some (§ *Eucervus*) developed, and in some (§ *Cariacus*) suppressed.

The duplication of the prongs was not taken cognizance of.

The author remarked that the facts thus summarized would be found published in more detail in the popular natural history periodical entitled "Field and Forest" for August, 1877.

129TH MEETING. OCTOBER 27, 1877.

Vice-President TAYLOR in the Chair.

Mr. J. E. HILGARD made a communication on

STANDARD SCALES, OR MEASURES OF LENGTH;

in which he stated that, having had occasion lately to compare with each other the two English standard yards, Nos. 11 and 58, made respectively of bronze and iron, deposited in the office of United States Coast Survey, they differed by an amount which, though very minute, was too considerable to be attributed to original errors of comparison. These two yards were presented to our Government in 1856 by that of Great Britain as exact

copies of the restored standard replacing the one destroyed by the burning of the Parliament building in 1834. The bronze standard is now found to be shorter than the iron one by about the quarter of the thousandth part of one inch. On subsequently comparing the duplicate bronze standard deposited with the Canadian Government, the same change was detected. This was referred to the bronze composition being in a state of tension owing to the varying molecular deportment of the copper, the zinc, and the tin, of which the alloy was composed, and the annual oscillations of temperature from summer to winter (amounting probably to nearly 100 degrees) to which the standards had been exposed so many times, in the small fire-proof building where they had been deposited for safe keeping. It was supposed that the iron standard, from being internally more homogeneous, was probably the more accurate measure of the two. Reference was made to various other alloys which had been proposed or employed; as in the new International standard metre of platinum and iridium; but the suggestion was offered that all were open to suspicion. Reference was also made to the employment of a natural crystal of quartz, 42 inches long, on which it was proposed to mark a standard metre.

Remarks on the communication were made by Messrs. ANTISELL, ELLIOTT, and TAYLOR.

A communication was made by Mr. E. B. ELLIOTT on

STANDARDS OF TIME—INTERNATIONAL, SECTIONAL, AND LOCAL.

The importance was urged in the first place of fixing more definitely in the Pacific Ocean, the dividing meridian separating the adjacent days of the month or week as computed from the eastern or the western arrival. Behring's Strait, the treaty boundary line between Alaska and Eastern Siberia, a meridian about 168½ degrees west from Greenwich, was referred to as a good natural terminus. The meridian of 180 degrees—some eleven degrees further west—was also referred to as one commonly used by navigators.

In this connection some remarks were made on the importance for railway and telegraphic purposes, of an International standard combined with sectional standards of time to replace the gradu-

ated local standards of time. As regards sectional time, it was suggested that exact hours from Greenwich would be found practically the most convenient. Thus instead of Philadelphia time, or St. Louis time, or Denver Mint time, or San Francisco time, we would have Atlantic slope time to be computed exactly five hours, Mississippi Valley time exactly six hours, Rocky Mountain time exactly seven hours, and Pacific slope time exactly eight hours from Greenwich. This would involve no change in watches or time-pieces, unless in the modification of the hour index. But the minute hands would continue in every case to indicate the actual time correctly.

REPORT OF THE COMMITTEE TO COLLECT INFORMATION RELATIVE TO THE METEOR OF DECEMBER 24TH, 1873.

(READ APRIL 7, 1877.)

At the meeting of the Philosophical Society on the 27th of December, 1873, its attention was called by Dr. PETER PARKER to a remarkable meteor which occurred on Christmas Eve, who described it in a note of the 25th December, given in the accompanying appendix, No. I.* Other members of the Society had also witnessed the meteor, and much interest having been awakened, the President, Prof. HENRY, suggested that "a committee be appointed to secure all the information possible to be obtained in reference to the unusually bright meteor." A committee was accordingly appointed, and they now submit this report of the manner in which the duty assigned has been discharged, and the results of their labors.

The committee began its labors by the formation of the chart No. 1, accompanying this report—embracing the country south of the centres of the states of Ohio and Pennsylvania, and north of central Georgia, within which region it was presumed the meteor might have been visible.

The following circular was then prepared and sent to about five hundred persons, especially to the postmasters and newspaper editors at county seats, to the colleges, and to meteorological observers :—

WASHINGTON, D. C., Jan. 9, 1874.

DEAR SIR : At a recent meeting of the Philosophical Society of Washington, it was resolved that a committee be appointed to secure all the information possible to be obtained in reference to the unusually bright meteor seen throughout this region of the country at about a quarter before eight on the 24th of December, or Christmas Eve.

To this end, I can assure you that the Society will highly appreciate your kindness if you will communicate to it such observations as were made either by yourself or your acquaintances, or such as may be found in records that are accessible to you, even if such observations are only of the most general character.

It is considered particularly important that you should give as detailed answers as possible to the following questions :—

* Owing to its great length this Appendix is not printed. Dr Parker's observations are, however, given in abstract in No. 22 of Appendix No. II. The publication of Chart No. 1 is regarded as unnecessary.

1. What was the exact time at which its appearance and disappearance was noted?
2. Is the time quoted by you railroad time or local time? if it is given by a clock that is regulated by a noonday mark, do you make an allowance for the equation of time as given in the almanac?
3. What was the bearing or azimuth of the meteor when first seen, and when last seen?
4. What was its apparent angular altitude when first and last seen?
5. What was its apparent angular altitude when at its highest above the horizon, or how great was its nearest approach to your zenith, and what was its bearing at that time? if you cannot satisfactorily state the angular altitude, give the *distance and height* of a tree, house-top, hill-top, or other object near which it passed.
6. For how many seconds was it visible, and did it appear to move more rapidly at one part of its path than at another?
7. How bright did it appear to be as compared with the brightest full moon, or the brightest stars?
8. Name the stars, if known to you, near which its path lay in the heavens?
9. Did it appear to be a bright point merely, or to have a sensible size? If the latter, state the apparent shape of the body, its length and breadth as compared with the diameter of the moon.
10. Describe the colors of the body, and the nature of its train, if any appeared in its wake?
11. How many seconds elapsed after the disappearance of the meteor before you heard the sound that accompanied it?
12. Describe as accurately as possible the nature of this sound, whether it was a single concussion like the discharge of a cannon, or a series of rolling sounds like the discharge of artillery or musketry; and if the latter, for how many seconds did the noise continue audible?

In explanation of the above questions, it may be stated that this meteor is believed to have passed in a westerly direction through the atmosphere at an elevation of a few miles above the earth's surface, and directly over the central portions of Virginia. Therefore to observers north of this region, as, for instance, at Baltimore, the meteor appeared some distance to the south of the observer's zenith; whilst to more southern observers it appeared to the north of their zenith, and, for instance, at Charleston, S. C., must have been seen, if at all, near the northern horizon.

You will confer a favor upon the Society by communicating this circular letter of inquiry to the people of your neighborhood, to the more intelligent of your employés, and to all persons of

accurate habits of observation, and especially by calling attention to this matter on the part of editors of local newspapers.

Any information on the subject communicated to the Society through myself or any member of the committee will be responded to in due season by the return of a small pamphlet containing a digest of all the information that may be gathered on the subject.

Very respectfully yours,

CLEVELAND ABBE.

On behalf of the following committee:
Hon. PETER PARKER, W. L. NICHOLSON, CLEVELAND ABBE.

The answers received from these persons, together with copies of the observations reported by the Smithsonian and the Signal Service observers, and other information copied from newspapers, are given in the accompanying Appendix No. I, which contains all the original documents that we have received relating to the appearance of the meteor.

Our thanks are especially due to the Postmaster-General, to the Secretary of the Smithsonian Institution, and to the Chief Signal Officer of the army for facilities afforded in the prosecution of this work.

Of the observers themselves, special mention should also be made of Professor R. L. Brackett, Westminster, Maryland; H. Inman, of Ellsworth, Kansas; Professor E. S. Holden, of the United States Naval Observatory; L. S. Abbott, Falls Church, Virginia; the Waterford Literary Society, of Waterford, Virginia; and S. Simpson, Fairfax Court House, Virginia; all of whom have put themselves to considerable trouble in order to materially further our investigation.

Notwithstanding this hearty co-operation, your committee have to regret that so little has been attained in the way of scientific exactness in respect to the path and dimensions of the meteor.

A large portion of the territory covered by our inquiries seems to have been overcast by clouds at the time of the meteor's passage, and although observations were indeed reported for about forty (40) localities, yet most of these (as the Society is well aware is usually the case) proved quite conflicting, or very indefinite. In the cases especially named above, a comparatively reasonable degree of accuracy was attained by the personal efforts of our correspondents, and in respect to the observations at Washington, D. C., by the personal examination of the observers by members of your committee. It is, in general, evident that satisfactory investigations of this sort can only be conducted by personal visitation of various localities by duly qualified individuals.

With these observations collected, all of which, with a few exceptions, were received during the year 1874, our investigation would have closed, and our results would have been reported

promptly to the Society, had not one of our members been led to believe that the disappearance of the meteor above Fairfax County, Virginia, or a little to the southwest thereof, might lead to the recovery of some fragments.

We have, therefore, for the past two years, sought opportunity of realizing this hope, which we now reluctantly relinquish, especially since the individual whose observations and conversation gave rise to this hope has removed to the State of Michigan during the past year.

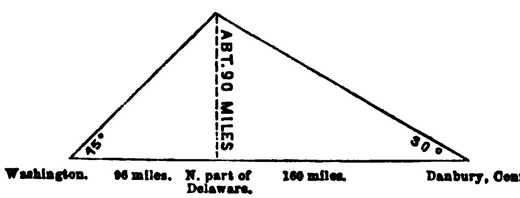
The following general account embraces most of what is deducible from the data that we have collected:—

The meteor appeared on Christmas Eve, December 24th, 1873, at 7 h. 39 m. P. M., Washington time; the "explosion" was heard at Washington at 7 h. 42 m. P. M.

In brightness it appeared to all (or nearly all) to exceed the full moon, and those who looked steadily at it state that the main body was of a conical shape, moving base forward (one says apex forward), with a short tail behind. No one speaks of any train being left behind, such as so often accompanies these meteors. The prevailing color was, we judge from the descriptions, a bright yellow, but the sparks or flames reported as proceeding from it were bright red and blue.

It entered the earth's atmosphere at some point vertically above the northern part of the State of Delaware, so that its apparent altitude as seen at Danbury, Conn., was 30°, and at Washington, D. C., about 45°, and its real altitude, therefore, above the earth's surface must have been about ninety miles.

Its path was from this point downward toward a point about eight miles above Fairfax Court House, Fairfax County, Virginia, after which it probably passed but little to the southwest before it disappeared; it may have passed as much as forty miles further, or to over Rappahannock County in the same State, but more likely is it that it disappeared when still over Fairfax County.*

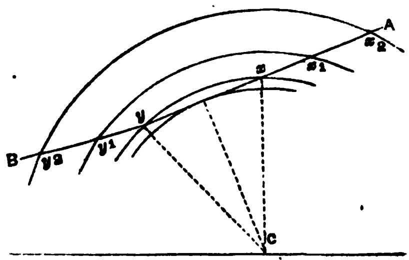


Its nearest approach to the City of Washington may perhaps be determined by the length of time elapsing before its sound

* The data recently collected by Prof. Chickering increase the probability that the meteor passed beyond Fairfax County, and was twenty miles above the earth's surface at its nearest approach.

was heard, which was 145 or 150 seconds, corresponding to a distance of thirty or thirty-one miles. We are disposed to consider the so-called "explosion" and subsequent "rumbling" not as due to a definite explosion of the meteor, but as the result of the concentration at the observer's ear of the vast volume of sound emanating, almost simultaneously, from a large part of the meteor's path, being, in that respect, not dissimilar to ordinary thunder.

It is evident that the waves of sound starting from that portion of the meteor's path nearest to the observer would reach his ear first; thus the observer at C would hear a violent sound due to the combined noises of the slight explosions, whirring, rushing, and snapping, emanating within a portion of a second from the whole of the line $x\ y$. Subsequently he would hear the sounds from $x\ x_1$ and $y\ y_1$, then those from $x_1\ x_2$ and $y_1\ y_2$, etc. The absorption and refraction of sound by the atmosphere and the progression of the meteor materially affect this phenomenon.



The dimensions of the cone-shaped meteor were estimated by Washington observers at about 45′ broad by 75′ long, from which some allowance is to be made for irradiation; but which correspond to 2000 and 3400 feet respectively.

By these figures, however, we are not to understand that there necessarily was any solid body of this size. The actual quantity of solid luminous matter producing the appearance of so large a meteor, as is well known, may have been extremely small; since the same visible effect would be produced by a cloud of very small and very bright meteoric particles moving very rapidly.

Of the actual velocity of the meteor there does not appear to be any means of forming a reliable determination. It probably described its entire visible path (of possibly about 120 miles) in three to five seconds.

For the convenience of those members who may desire to submit to further study the data we have collected, we submit, in Appendix No. II, some convenient abstracts of the more important reports.

In Appendix No. III we nave collected the data relating to the position of the meteor at its disappearance.

In Appendix No IV will be found whatever seemed valuable with reference to the sounds heard during or after its passage.

20

In Appendix No. V are given in classified arrangement the observations relating to the appearance of the body and its train.

Besides Chart No. 1, already referred to, we also give in Chart No. 2* the four sheets of the large Post-Route Map of the State of Virginia and parts of adjacent States, on which are underscored, in red, the stations from which reports have been received.

PETER PARKER,
CLEVELAND ABBE,
W. L. NICHOLSON, } *Committee.*

WASHINGTON, 7 April, 1877.

APPENDIX No. II.

ABSTRACTS OF REPORTS AND LETTERS.

In the following abstracts of the letters and other documents that have been received relative to the Meteor of Christmas Eve, the phenomena that were observed have been classified as follows:—

1. Time of occurrence.
2. Duration of the visible flight of the meteor.
3. Apparent bearing.
4. Apparent altitude.
5. Apparent brightness, size, and color.
6. Interval between the light and the sound.
7. Nature of the sound.

The pages quoted in these abstracts refer to the collection of original letters in manuscript Appendix No. I.

1.

H. C. RYDER, Danbury, Fairfield Co., Connecticut, pp. 89 to 93, and 115.

3. Disappeared at south, $57\frac{1}{2}°$ west; true bearing.
4. Started at 30°; path inclined 20 or 30° downward to the right; ended at 5°.
5. White; much brighter than the moon.

6.
7. } Saw no explosion, heard no sound.

* This chart reduced follows page 161. The stations from which reports have been received are marked upon it.

2.

Anonymous, Mercersburg, Franklin Co., Pennsylvania, pp. 23 to 25.

1. At 7.30 or 7.45 P. M.
2. Two or three seconds.
3. First in the east; last in the south.
4. First at 45°; last behind the U. P. Church.
5. Like red-hot iron; round, with train of sparks.
7. Noise accompanying its passage like that of a hissing fire-cracker, or a loud hiss; no noise or explosion of any kind at the close.

3.

R. F. Snoddy, Shippensburg, Cumberland Co., Pennsylvania, p. 39.

1. 8.30 P. M., railroad time.
2. Not 2 seconds.
3. First in the east; last in the southwest?.
5. Like the sun, and white.
6. About 2 seconds.
7. Rushing noise with rolling sounds like an approaching thunder-storm.

4.

W. F. Madlem, Ephratah, Lancaster Co., Pennsylvania, p. 237.

1. 7.47 P. M.
2. 15 seconds.

5.

Prof. G. R. Rossiter, Marietta College, Ohio, p. 65, could not learn that it had been seen by any one in that region.

6.

F. A. Curtis, Newark, New Castle Co., Delaware, pp. 1–3, 233, A. and B.

1. A few minutes before 8 P. M., or 7.45 P. M., Philadelphia railroad time.
2. 30 seconds.
3. It passed very near the moon, a little south.
4. Overhead, a little to the south, "say, 30°"; last lost in haze near the horizon.
5. Much brighter than the moon; a succession of flashes of white light formed the tail; the head was of different colors, crimson predominating.

7.

J. J. BLACK, New Castle, New Castle Co., Delaware, pp. 45–48.

1. 7.55 P. M., Philadelphia time.
2. 10 minutes.
3. First in the west; last in the west.
4. First, 65°; last, 50° or 40°.
5. Nearly equal to full moon in size and brightness. Burst into three pieces with a yellow train; head of many colors, principally red.
7. Like a subdued rushing sound.

8.

MESSRS. WOOD, SMITH, and OTHERS, through R. H. GILMAN, Milford, Kent Co., Delaware, pp. 79–81.

1. Between 7.30 and 8 P. M.
3. First in the south; last at the place where the sun set.
4. First, 45°; last, 0°.
7. Noise like distant cannon.

9.

R. H. GILMAN, Milford, Kent Co., Delaware, p. 165.

1. 8 P. M.
3. In the west.
5. Bright ball; large as one's head.

10a.

A. C. CORRESPONDENT OF BALTIMORE AMERICAN, p. 155.

1. 7.58 P. M.
3. Moved toward the south by west, veering southward.
4. First altitude was 48°
5. Grew more brilliant toward the close; encircled with rings of green and orange.
7. Followed by a sound like 100-pound Parrott conical shells.

10b.

EDITOR OF BALTIMORE NEWS; CORRESPONDENT P. OF BALTIMORE AMERICAN, Baltimore, Maryland, pp. 153 and 155.

4. Passed under position of the moon.
5. Three times the size of Venus.
7. No sound heard.

11.

W. Allan, McDonogh's Institute, near Owing's Mills P. O. (12 miles northwest of Baltimore), Baltimore, Maryland, p. 79.

3. First in the southwest; last, further west.
4. First, 45°; last, 25°.
5. Shadows overcame moonlight and gaslight; the diameter not much less than the moon; left bright train behind it.
7. No noise heard.

12.

E. L. Huntt, Mattawoman, Charles Co., Maryland, pp. 33 and 34.

1. About 8 P. M.
2. 2 or 3 seconds.
3. First in the southeast; last in the northwest.
6.-7. Just as it disappeared, followed by a rumbling sound like the firing of cannon.

13.

R. L. Brackett, Western Maryland College, Westminster, Carroll Co., Maryland, pp. 247–249, 257–259. Reliable.

3. Some time before disappearance it bore south 30° west; and by another person, south 33° west, at the time of disappearance; and by a third, less reliable, 29°.
4. Just before disappearance, 12°.
5. Half size of full moon.
6. 127 seconds.

14.

Prof. R. L. Brackett and others (Western Maryland College), Westminster, Carroll Co., Maryland, pp. 247–249. Less reliable.

1. 7.45, or 7.50 P. M.
2. 4 or 5 seconds.
3. First in the southwest by west (or 60°).
4. First 40°.
5. Much brighter than full moon; of a bluish tint, red, steel-blue, and white heat; shape of head an elongated ball, followed by a bright train.
6. 2 minutes.
7. Very distinct, like a cannon discharge, followed by a series of sounds for two or three seconds.

15.

M. Shellman, A. H. Huber, and others, Westminster, Carroll Co., Maryland, pp. 41–44.

1. About 7.40 P. M., local time
3. Last in the southwest.
5. Brighter than the moon; color soft, silvery, with a bluish cast.
7. A concussion, producing a slight jarring of the windows.

16.

C. H. Jourdan and others (Mt. St. Mary's College), Emmittsburg, Frederick Co., Maryland, pp. 59–61.

1. 7.45 P. M.
4. South of the zenith.

17.

H. H. Hopkins, New Market, Frederick Co., Maryland, pp. 121–123, 165.

1. 8 P. M., railroad time.
2. 6 seconds.
3. First in the southeast, last southwest (?) or south (?).
5. Much brighter than full moon; head ¼ diameter of the moon; color bright-red and yellow.
6. 4 or 5 seconds.
7. Noise like the discharge of a cannon.

18.

W. H. Zimmerman, J. W. Greenwood, and others (Washington College), Chestertown, Kent Co., Maryland, pp. 83–88.

1. Between 7.30 and 8.00 P. M.
2. 5 or 6 seconds.
4. First 30°; path oblique to the horizon, toward the west by south.
5. Bright as full moon; light white, with a reddish glow; train about 10° long.

19.

B. Hallowell and others, Sandy Spring, 18 miles north of Washington, Montgomery Co., Maryland, pp. 173, 183–186.

2. 2 seconds. Reliable.
3. South.
4. 50°.
6. 2 minutes. Reliable.

20.

H. C. HALLOWELL, Sandy Spring (18 miles north of Washington), Montgomery Co., Maryland, pp. 153, 187, 189.

1. About 8 P. M.
3. First in the south; last, southwest.
4. First, 50°; last, 10°.
5. Body ⅛ size of full moon; of elongated shape, and of intense greenish-white light, with the head of red and blue, with scintillations, followed by trail 3° long; shadows overcame lights in the house.
6. Interval from 1½ to 4 minutes.
7. Noise, a sharp report that shook the windows.

21.

J. A. HOPKINS, 121 Pennsylvania Avenue, Washington, D. C., pp. 5, 135.

2. 4 seconds.
7. Like rattling of a door.

22.

DR. PETER PARKER, Corner 11th and Pennsylvania Avenue, Washington, D. C., p. 19.

1. 7.30 P. M.
5. Wedge-shape, with pale violet point; like a vibrating veil of light.

23.

H. INMAN, Corner of Pennsylvania Avenue and 13th Street, Washington, D. C., pp. 95-99, 163.

1. 7.42, minus 4 minutes? (7.38 Washington time).
2. 20 seconds.
3. First directly over the Capitol dome [E. 20° S.]; last, due west.
4. First, about 45°; passed within 5 or 10° south of the zenith; disappeared behind a building; altitude less than 5°.
5. Yellow base, 1½ times the moon's diameter; length 2½ times the moon's diameter; apex dull; moved with its base in front.
6. 45 seconds after the disappearance.
7. A dull thud.

24.

DR. T. H. GILL, Corner of 10th and Canal streets, Washington, D. C., p. 151.

6. 145 or 150 seconds between the first sight and the explosion. [Reliable.]

25.

W. J. McIntire, Corner of 12th and Pennsylvania Avenue, Washington, D. C., p. 159.

5. White, hot; front, blue; rear, dark red; general effect blue; body well defined, blunt head; breadth, one moon's diameter, and length, three moon's diameter.
7. Whizzing sound like bombshells.

26.

Isaac Lynch, Farragut Square, Washington, D. C., pp. 179–182.

1. 7.50 P. M.
2. 3 or 4 seconds.
3. Due south; last azimuth 60° west, or a little south of the moon.
4. 60°; last, 25°.
5. Bright as a street-lamp at 20 feet; diameter, ½ moon with tail 3 moons; clear white, with rays of colors.
6. 25 seconds.
7. Explosion followed by rumbling like thunder; continued 20 seconds.

27.

J. H. L. Eager, New Jersey Avenue and B Street, southeast, Washington, D. C., p. 227.

1. 8.40 or 7.40 P. M.
3. First, east; second, west.
4. Last altitude about 22°.
5. Pure white.

28.

E. S. Holden, U. S. Naval Observatory, Washington, D. C., pp. 239–246.

1. 7.40.
2. About 2 seconds.
3. Last, south 68° west. Careful determination.
4. Last altitude, 4° 45′, or less. Careful determination.
5. More yellow than moonlight, and cast distinct shadows.
7. No noise heard.

29.

Professors Newcomb, Hilgard, and Baird, Washington, D. C., pp. 199, 200.

6. Newcomb estimates it at 1½ minutes; had his eye at telescope.
7. Hilgard heard it within a closed room; Baird heard rattling of windows at 7.42.

30.

J. D. Mitchell, A. McLean, and others, Alexandria, Fairfax Co., Virginia, pp. 15, 159.

1. From 7.30 to 8 P. M.
3. Westward along King Street, inclining to the south.
4. Directly overhead.
7. Heavy concussion; house trembled.

31.

L. S. Abbott, Falls Church, Fairfax Co., Virginia, pp. 67–77, 157, 167, 209.

4. First, 75° south; course west; disappeared at 45°.
5. Round red head, followed by a train and surrounded by diverging rays, flashing white and very brilliant.
6. Interval very long, from 60 to 90 seconds.
7. Like very heavy cannon, followed for 30 seconds by rolling, rumbling sounds, fading away in the east, as it came back on the track of the meteor.

32.

S. Simpson, J. Mills, and others, Fairfax C. H , Fairfax Co., Virginia, pp. 251–256.

1. 7 to 8 P. M. About 8 P. M.
2. 2 or 3 minutes.
3. First in the east; last in the west.
4. First, 45°; last, 45°.
5. Equal daylight, head brighter than its red tail and broad as the moon.
6. Interval 3 or 4 minutes, or time enough to walk 150 yards.
7. First like a cannon, jarring all the houses in this section, then rumbling for 4 or 5 minutes; dying away in the east.

33a.

Fairfax Court House, Fairfax Co., Virginia, p. 157.

1. About 7 P. M.
5. Bright light.
6. A few seconds.
7. Shock that shook the ground and rattled windows, and succeeded by a rushing noise as of wind in a forest.

33b. Vienna, p. 157.	About 7.30 P. M. a loud clap of thunder and a vivid flash of light seen in all this neighborhood.
33c. Falls Church, p. 157.	
33d. Langley, p. 157.	
33e. Lewisville, p. 157.	

34.

CENTREVILLE, Fairfax Co., Virginia.

4. Said to have exploded in the zenith.

35.

C. GILLINGHAM, Woodlawn, Accotink P. O., Fairfax Co., Virginia, p. 165.

1. 7.30 P. M.
3. From east to west.
4. 45° at its disappearance.
5. Exceeded bright coal-oil lamp.
7. Short, hard reports like heavy cannon, and continued resounding for considerable time.

36.

EDITOR OF THE DISPATCH, Richmond, Henrico Co., Virginia, p. 157.

1. About 7 P. M.
3. Moving from east to west.
4. Directly over the city.

37.

T. CHRISTIAN, Richmond, Henrico Co., Virginia, pp. 207–210.

3. First near the northern horizon; did not see the beginning or end.
4. Last seen at 9° 30′, but did not see the end. Path inclined 10° to the horizon.
5. Brighter than full moon.

38.

REV. H. BRANCH and OTHERS, between Hamilton and Waterford, Loudoun Co., Virginia, pp. 141–150.

Meteor was seen.

39.

WATERFORD LITERARY SOCIETY, Waterford, Loudoun Co., Virginia, pp. 3–55.

2. About 5 seconds.
3. First a little north of east; last, southwest.
5. Not quite equal to the moon; brilliant white; pear-shape; bluish color.
7. Like train of cars approaching a tunnel.

40.

W. D. CHRISTIAN, Appomattox C. H., Appomattox Co., Virginia, pp. 171–172.

1. 8 P. M.
2. 3 or 4 seconds.
3. First in the north 55° east; last, north 25° east.
4. First, 40°; last, 20°, behind trees.
5. ⅘ of the moon.
7. No sound, unless like a distant gun.

41.

A. P. BROWN and OTHERS, Upperville, Fauquier Co., Virginia, pp. 221–224.

1. 8 to 8.15 P. M.
2. 3 or 4 seconds.
3. First, east-northeast; last, east-southeast.
4. First altitude 70°; last altitude 45°. [Reliable.]
6. 15 seconds.
7. Like distant cannon, or thunder.

42.

GEORGE D. SMITH, Marion, Smyth Co., Virginia, pp. 201–206.

3. First in the east; second, southwest.
4. First (?) passed near the zenith; last, 30°.
5. Burst into red, white, blue fragments, at 30° altitude.
6., 7. No sound.

[This appears to refer to some other, perhaps the 11 P. M. meteor.]

43.

T. E. CONVERSE, Woodstock, Shenandoah Co., Virginia, p. 235.

1. At 7.45 P. M.
2. 3 to 6 seconds.
3. First, due east; last, in the east.
4. First, 45°; last 20° or 25°.
5. Thrice the brightness of the moon; white; no special shape.
6., 7. No sound.

44.

W. ALGER and G. H. MATHER, Ousley's Gap, Cabell Co., West Virginia, pp. 140 and 170.

1. Cloudy and nothing seen or heard. (Describes the meteor of January 6.)

45.

J. F. HARTGROVE, Harper's Ferry, Jefferson Co., West Virginia, pp. 101–106.

1. 7.45 P. M. (Probably not noted exactly.)
2. Visible during nine breaths (about 30 seconds ?).
3. Observer standing on Shenandoah Street, 100 yards from the junction of the Shenandoah and Potomac rivers, facing the southeast; when first seen, meteor bore a little south of the Potomac River; it was approaching the earth and moving southwestward; when it exploded it bore south-southwest; did not pass west of the Blue Ridge.
5. Brighter than the moon, and ¼ of its diameter; it cast a shadow; explosion followed by falling vertical streaks: color, white and blue.
6. Interval between visible explosion and audible sound, three breaths (10 seconds).
7. Hissing sound during the whole apparition; explosion equal to a single concussion of a well-charged musket.

46.

J. J. BARRICK, New Creek (Keyser P. O.), Mineral Co., West Virginia, pp. 49–51.

1. 7.42, railroad time.
2. 5 seconds.
3. Passed near Orion, from a little north of east to a little south of west.
4. Greatest altitude equal to altitude of sun at 9½ A. M.
5. Well defined; sensible size equal ⅔ of the full moon; light equal to that of the full moon.
7. No sound; saw it explode; soon afterwards it began to descend.

47.

J. T. MCCREENEY, G. H. PRINCE, and OTHERS, Raleigh C. H., Raleigh Co., West Virginia, pp. 35 and 57.

1. Nothing seen.
7. Noise heard to the southward, as of distant thunder; remarked by many citizens.

48.

J. L. GOULD, Buckhannon, Upshur Co., West Virginia.

1. Few minutes before 8 P. M.
3. First, in the east; second, east, 5° north.
4. First, 40 or 50°; second, 20° or less; path downwards and inclined 8° to the north of the prime vertical circle.

49.

C. H. J. RAWLING, Wheeling, West Virginia, p. 195.

1. Nothing seen or heard.

APPENDIX No. III.

POSITION OF THE METEOR AT ITS DISAPPEARANCE.

Observations relating to the end of the visible path of the Meteor.

The following notes give all the information that has any claim to accuracy with reference to the place of disappearance of the meteor. (The longitudes here given are supposed to refer to the dome of the Capitol at Washington.)

STATION NO.

(1). H. C. Ryder, lat 41° 20', long. 3° 35' E.
Meteor disappeared at altitude 5°, bearing S. 65° W., as measured with compass, and allowing for variation.

(6). F. A. Curtiss, lat. 39° 38', long. E. 1° 20'.
Disappeared in the haze in the S. W. horizon.

(7). J. J. Black, lat. 39° 40', long. E. 1° 24'.
Burst in three parts at a zenith distance 40° toward W.

(9). R. H. Gilman and others, lat. 38° 56'. long. E. 1° 38'.
Disappeared where the sun set (or S. 68° W.).

(11). W. Allan, lat. 30° 27', long. E. 0° 15'.
Altitude 25° when it disappeared [possibly hidden by trees].

(18). R. L. Brackett, lat. 39° 35', long. E. 0° 5'.
a. Disappeared at alt. 12°, bearing S. 30° W. by theodolite, as observed by Mr. Middleton.
b. Disappeared bearing S. 33° W., as observed by Mr. White.
c. Disappeared bearing S. 29° W., as observed by Mr. ———.

(19). H. C. Hallowell and others, lat. 39° 9', long. E. 0° 2', elevation 600 feet.
Disappeared in S. W. about 10° above the horizon.

(21). Mrs. J. A. Hopkins, lat. 38° 54', long. W. 0° 0'.
Disappeared bearing S. 88° 15' W.

(23). H. Inman, lat. 38° 54', long. W. 0° 1'.
Was standing at the corner of Penn. Avenue and

Fourteenth Street; saw meteor disappear behind some trees and buildings at an altitude less than 5°, bearing directly W. (Three estimates and measurements respectively gave S. 89° W., and N. 88° W., and due W.).

(27). J. H. L. Eager, lat. 38° 54′, long. 0° 0′.
Disappeared nearly due W.

(28). E. S. Holden, lat 38° 54′, long W. 0° 2.5′.
Disappeared bearing S. 68° W., alt. 4° 45′ or less.

(30). J. D. Mitchell and others, lat. 38° 48′, long. W. 0° 3′.
Passed directly overhead westward, or S. S. W., and disappeared.

(31). L. S. Abbott, lat. 38° 53′, long. W. 0° 13′ [1½ miles W. of Falls Church].
Disappeared in W. quietly and instantly, at alt. 45°.

(32). Joseph Mills, near Fairfax C. H., lat. 39° 41′, long. W. 0° 19′.
Disappeared in W. at alt. 45°.

(34). Anonymous, Centreville, Fairfax County, Va., lat. 38° 40′, long. 0° 26′ W.
Meteor well seen, and exploded in the zenith. [This may mean that the meteor passed through the zenith, and that an explosion was afterwards heard.]

(35). C. Gillingham, Woodlawn (½ mile S. W. of Mt. Vernon), Accotink, Fairfax County, Va., lat. 37° 42′, long. 0° 18′ W.
Passed from E. to W.; disappeared at alt. 45°.

(37). T. Christian, Richmond, Henrico County, Va., lat. 37° 32′, long. 0° 24′ W.
The path was westward and downward; inclined 10° to the horizon; was last seen at alt. 9° 30′; did not see the end.

(40). W. D. Christian, Appomattox C. H., Appomattox County, Va., lat. 37° 20′, long W. 1° 51′.
First seen bearing N. 55° E., alt. 40°; last seen behind trees N. 25° E., alt. 20°.

(41). A. P. Brown and others, Upperville, Fauquier County, Va., lat. 38° 59′, long. W. 0° 53′.
First seen alt. 70°, bearing considerably N. of E.; last seen alt. 45°, bearing considerably S. of E.

(42). G. D. Smith, Marion, Smyth County, Va., lat. 36° 48′, long. W. 4° 31′, elev. 2400 feet.
Meteor burst into fragments at alt. 30° in the E. [This may have been the 11 P. M. meteor.]

(43). T. E. Converse, Woodstock, Shenandoah County, Va., lat. 38° 50′, long. W. 1° 31′.
First seen at alt. 45°, bearing due E.; last seen alt. 20° or 25° in the E.

(45). J. F. Hartgrove, Harper's Ferry, Jefferson County, West Va, lat. 39° 18′, long. W. 0° 43′.
First seen bearing E. 10° S., rapidly approached the earth, and exploded bearing S. S. W.? (or S.?)

(46). J. I. Barrick, New Creek (Keyser P. O.), Mineral County, West Va., lat. 39° 27′, long. W. 1° 55′.
Observed at 7 h. 42 m. railroad time. First alt. 18°, bearing ——, saw it explode.

(48). J. L. Gould, Buckhannon, Upshur County, West Va., lat. 38° 56′, long. W. 3° 9′.
Looking E. observed meteor falling from alt. 60° to alt. 20° or less; path not vertical, but deflected 8° northward.

APPENDIX No. IV.

THE SOUNDS ACCOMPANYING AND FOLLOWING THE METEOR.

One of the most interesting subjects in connection with such large meteors relates to the sound, as of an explosion, that is frequently observed, and which generally reaches the observer at a considerable interval of time after the body has disappeared from view.

It is not, however, necessary to assume that an actual explosion must have occurred at some portion of the meteor's path. The correct explanation of the phenomenon is undoubtedly found in the fact that, besides the rushing of air and the singing or whizzing of a revolving projectile, there is a series of small noises of the nature of explosions, and like the snapping, hissing, and crackling of a wood fire, or soft coal fire, attending the burning of the outer surface of the meteor.

Owing to the rapid movement of the body, these noises are produced in a few seconds, and, as it were, almost simultaneously, from one end to the other of a line many miles in length.

Now, if the observer be so situated that the noises from a large portion of the path reach him, almost simultaneously he will hear one loud noise attended by a succession of rapidly diminishing sounds.

The problem, in fact, is precisely similar to that which has excited so much interest in Optics, namely, as to the effect of the movement of the observer and the source of light upon the apparent color and intensity.

So far as concerns the acoustic problem, it has been theoretically investigated by Eotvos [Poggendorf, Annalen 1875, clii., p. 535].

This author has shown that his theoretical formulæ agree closely with the results of experiments made upon rapidly-moving railroad trains. We have, however, in the case of a meteor, far greater relative velocities than any that have been artificially produced for experimental purposes, and had we any accurate measurements, we might be able to learn something with regard to the accuracy of theoretical investigations, or might possibly learn something with regard to the absolute intensity of the individual noises occurring at the surface of the meteor.

Failing to obtain accurate data, we may remark that it requires only comparatively feeble noises distributed along the entire path of the meteor in order to produce, by their concentration at the observer's station, a sound equal to that of loud thunder.

In the following table we have collected most of the data relating to the noises that accompanied the meteor.

The following report to have heard no sound :—

(1). Danbury, Connecticut.
(4). W. F. Madlem, Ephratah, Pennsylvania.
(5). G. R. Rossiter, Marietta, Ohio.
(6). F. A. Curtis, Newark, Delaware.
(10*b*). Anonymous, Baltimore, Maryland.
(11). W. Allan, Owing's Mills, Maryland.
(16). C. H. Jourdan, Emmittsburg, Maryland.
(18). W. H. Zimmerman, Chestertown, Maryland.
(42). G. D. Smith, Marion, Virginia.
(43). T. E. Converse, Woodstock, Virginia.
(46). J. J. Barrick, New Castle, West Virginia.

Besides these, many others who heard no sound are omitted, since others from the same places heard the sounds distinctly, as described in the following abstracts :—

(2). Mercersburg, Franklin Co., Pennsylvania.
Noise accompanying its passage like a hissing fire-cracker. [Similar reports with reference to other meteors have usually been considered as an illusion on the part of the observer, as was probably the case also on the present occasion.]

(3). Shippensburg, Pennsylvania.
Interval 2 seconds; rushing, rolling sound, like an approaching storm.

(7). New Castle, Delaware.
Subdued rushing sound.

(8). Milford, Delaware.
Explosion like a distant cannon.

(10*a*). Baltimore ? Maryland.
Followed by sound like that of 100-pound Parrott conical shell.

(12). Mattawoman, Maryland.
As it disappeared, followed by a rumbling sound.

(13, 14, 15). Westminster, Maryland.
Interval of 127 seconds; noise like a cannon, jarring the windows, followed by a series of rumblings.

(17). New Market, Maryland.
Interval of 4 or 5 seconds; sound like a cannon.

(19, 20). Sandy Spring, Maryland.
Interval, 2 minutes; sharp report like a cannon, shaking the windows.

(21 to 29). Washington, D. C.
At 7.42 P. M. an explosion similar to a heavy cannon, shaking windows and doors, at an interval after the disappearance of the meteor, variously estimated; the most accurate observation being that of No. 24, T. H. Gill, which gives from 145 to 150 seconds; the explosion was followed for 20 seconds, or more, by a rumbling noise.

(30). Alexandria, Virginia.
Heavy concussion; house trembled.

(31). Falls Church, Virginia.
Interval, 60 to 90 seconds; noise like heavy cannon, followed for 30 seconds by rumbling, fading away in the east.

(32 & 33*a*). Fairfax Court House, Virginia.
Concussion that shook the ground; interval a few seconds (possibly 45 seconds, according to Mr. S. Simpson), followed by rumbling, rushing noises.

(33 *b*, *c*, *d*, *e*). Vienna, Virginia.
Like loud clap of thunder.

(34). Centreville, Virginia.
Explosion in the zenith.

(35). Accotink, Virginia.
Short, hard report, like heavy cannon.

(36 & 37). Richmond, Virginia.
Shook the ground and rattled windows.

(39). Waterford, Virginia.
Noise like train of cars.

(41). Upperville, Virginia.
Interval of 15 seconds; sound like distant thunder.

(45). Harper's Ferry, West Virginia.
During the whole apparition a sound like a sky-rocket, but twice as loud; after an interval of 3 breaths (equal to 20 seconds), came an explosion like a well-charged musket.

(47). Raleigh Court House, West Virginia.
Sound like that of distant thunder, to the southward.

APPENDIX No. V.

CLASSIFIED RÉSUMÉ

Of the particular observers' remarks as to the meteors' *apparent size and shape*, its *brightness relative to the full moon*, the *color of the body* or forward portion, and *that of the tail*, or following part.

The numbers attached to these remarks respectively refer to the several individual abstracts of this Appendix (No. II), beginning on page 144.

Apparent size and shape:

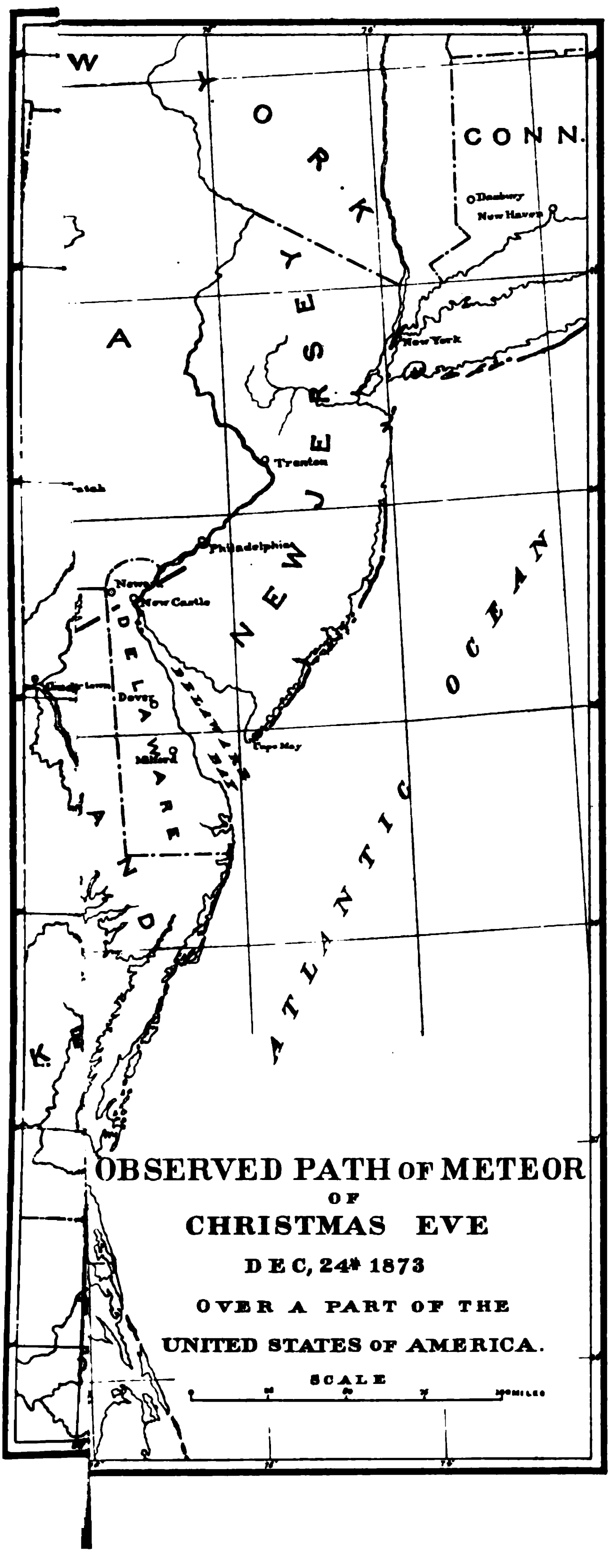
	STATION NO.
Equal to full moon	(7, 11, 32)
Two-thirds diameter of full moon	(40, 46)
Head half diameter of "	(13)
" one-fourth diameter of full moon	(17, 45)
" one-sixth " " " and of elongated form	(20)
" three times size of planet Venus	(10*b*)
Wedge-shaped, with pale violet point	(22)
Pear-shaped	(39)
Base 45′, moving forward; Length 75′	(23)
Base 30′; Length 90′	(25)
Base 15′; Length 90′	(26)

Brightness relative to full moon:

Brightness greater than that of full moon	(1, 6, 14, 15, 17, 37, 45)
" equal to full moon	(7, 18, 46)
" not quite equal to full moon	(39)
" three times that of "	(43)

Color:

Body: white	(1, 2 as red hot iron, 3 like sun, 27, 43)
" light white, with reddish glow	(18)
" clear white, with rays of colors	(26)
" white, or soft silvery, with a bluish tint	(14, 15)
" brilliant white, tail bluish	(39)
General color: white and blue	(45)
Body: yellow, apex dull	(23)
" yellower than moonlight	(28)
" predominant color *red* (or crimson)	(6 crimson, 7 red)
" bright *red and yellow*	(17)
Round *red* head, with diverging or flashing rays from it, of a white color	(31)



OBSERVED PATH OF METEOR
OF
CHRISTMAS EVE
DEC, 24th 1873
OVER A PART OF THE
UNITED STATES OF AMERICA.
SCALE

Body: red and blue	(20)
" general color *blue*	(25)
Tail (or train):	
Bright train following	(11)
Elongated ball, followed by a bright train	(14)
Tail about 10° long	(18)
Train white, but not so intense as the rays diverging from the head	(31)
Train of sparks	(2)
Successive flashes of white light	(6)
Tail 3° long, greenish-white	(20)
Body burst into three pieces with a yellow train	(7)

130TH MEETING. 7TH ANNUAL MEETING. NOVEMBER 10, 1877.

Vice-President HILGARD in the Chair.

Twenty-three members present.

The names of members elected since the last annual meeting were read.

The election of officers for the ensuing year was conducted in accordance with the rules, with the following result:—

President,	JOSEPH HENRY. (Unanimously.)
Vice-Presidents,	J. K. BARNES, W. B. TAYLOR, J. E. HILGARD, J. C. WELLING.
Treasurer,	CLEVELAND ABBE.
Secretaries,	J. H. C. COFFIN, T. N. GILL.

MEMBERS OF THE GENERAL COMMITTEE.

THOMAS ANTISELL, SIMON NEWCOMB,
E. B. ELLIOTT, J. W. POWELL,
ASAPH HALL, C. A. SCHOTT,
N. S. LINCOLN, J. M. TONER,
J. J. WOODWARD.

Standing Committees, appointed by the General Committee.

On Communications,	J. J. WOODWARD, J. E. HILGARD.
On Publications,	S. F. BAIRD, T. N. GILL, J. H. C. COFFIN, C. ABBE.

131st Meeting. November 24, 1877.

The President, Joseph Henry, in the Chair.

Thirty-six members and visitors present.

The President read his annual address, as follows:—

Gentlemen, Members of the
Philosophical Society of Washington.

I beg leave to tender you my sincere thanks for the honor you have conferred upon me, and the good feeling you have manifested towards me, by my re-election as President of this Society. I say the good feeling which you have manifested towards me, because I know that there are many of your members who can much more efficiently discharge the duties of the office than I can. I may, perhaps, be allowed to say, without the charge of undue egotism, that I have never occupied any position for which I have been voluntarily a candidate. The several offices of honor and responsibility which I now hold, no less than nine in number, have all been pressed upon me without solicitation on my part, and I now begin to feel that, in view of that peculiarity of human nature so admirably exhibited in the character of the Archbishop of Granada, that I ought to diminish the number of my responsibilities, gradually leaving to others the honor and the toil of office. It is, therefore, with no feigned hesitation that I again accept the re-election to the position to which your kindness has called me.

I have, however, taken from the first a deep interest in the Society, knowing that it is intimately connected with the intellectual development of the City of Washington, and that it has a reflex influence upon every part of the United States. It tends to keep alive an active spirit of scientific advancement, not only to diffuse a knowledge of the progress of discovery among its members, but also to stimulate by friendly criticism and cordial sympathy to new efforts in the way of explorations into the unknown.

While but comparatively few qualifications are necessary for admittance, yet no person is elected who is not supposed to have at least a high appreciation of science; has some familiarity with its principles, and is capable of doing something in the way of promoting the objects of the Association.

The general mental qualification necessary for scientific advancement is that which is usually denominated "common sense," though, added to this, imagination, invention, and trained logic, either of common language or of mathematics, are important adjuncts. Nor are objects of scientific culture difficult of attainment. It has been truly said that the "seeds of great discoveries are constantly floating around us, but that they only take root and germinate in minds well prepared to receive them."

The preparation, however, is not difficult, and many possess the requisites in an eminent degree who are not aware of the fact. Genius itself has been defined as a mind of general powers determined, enthusiastically it may be, on one pursuit.

The method of discovery or scientific observation is not difficult. There is a story in a work, entitled "Evenings at Home," which produced an indelible impression on my mind. It is entitled "Eyes and no Eyes," and related to two boys who started on a walk during a warm summer afternoon. On their return, one was fatigued, dissatisfied, having seen nothing, encountered only dust and heat, while the other was charmed with his walk, which had been over the same ground, and gave a glowing account of the objects which he had met with, and of the reflections which were awakened by them. On this story, De la Bêche has founded a work, entitled "How to Observe in Geology," which I would commend to the attention of every member of this Society, while I suggest that good service would be done to the advance of knowledge were a similar work published relative to all branches of science.

The first requisite for an observer is, that his mind should be actively awakened to the phenomena of nature with which he is surrounded. Thousands of persons of excellent mental capacity pass through the world without giving the slightest attention to the ever-varying exhibitions which are presented to them. The sun rises and sets, the seasons change, the heavens every night present new aspects, but these to them are matters of course; they excite no interest, and it is only when some extraordinary phenomenon occurs, such as the blazing comet or the startling earthquake, that their attention is arrested. Another requisite is the power of the perception of truth, which enables the observer to recognize and define with unerring accuracy what he has seen without any tinge of color from *à priori* conceptions. Still an-

other is the faculty of eliminating accidental conditions from those which are essential: and further, the characteristic of perseverance is indispensable.

The fields of scientific labor may be divided into two classes, viz.: those which relate to the empirical observation of facts, and those which refer to the systematic series of investigations relative to the law or cause of special phenomena. As illustrations of the first class are the facts of the phenomena of the physics of the globe, those of ordinary meteorology and natural history; while, as examples of the second, we have the phenomena of chemistry, physics, and astronomy.

The remarks which I have previously made relate principally to the former. In order to elucidate the method of investigation in the latter case, I will suppose the existence of a new phenomenon which is unconnected with any of the present generalizations of science, but of which it is desired to discover the law or the facts with which it is associated. Such facts standing alone form no part of science; they are usually discovered in the course of investigations, and are of great importance in pointing out fields of new research which promise an abundant harvest.

The first step in the investigation is to reproduce the phenomenon; the next, is to form in the mind a provisional hypothesis as to its cause, and in the choice of this we are governed by analogy. For example, if it appears to resemble some of the phenomena of electricity, we *assume* that it is produced by electricity; we next endeavor to ascertain by what known action of electricity such an effect could possibly be produced; for this purpose we invent an hypothesis, or imagine some peculiar action of electricity sufficient to produce the effect in question: we then say to ourselves, if this be true, it will logically follow that a specific result will follow if we make a certain experiment: the experiment is devised and tried, but no positive result is obtained. In order to this negative result, the logical deductions must have been in error, or the experiment must have been defective, or the hypothesis itself erroneous.

We examine each of the two former steps, and finding nothing amiss in them, we conclude that the hypothesis was not true: another hypothesis is then invented, another deduction inferred, and another experiment made; still no result is obtained. At this stage of the research the inexperienced investigator is prone

to abandon the pursuit; not so he who has successfully attempted to penetrate the secrets of nature. Undeterred by failure, he changes from time to time his hypotheses, makes new guesses, and again repeats the question as to their truth by means of experiment, until at length nature, as if wearied by his solicitations, grants him a new and positive result; he has now two facts, and an hypothesis to explain them; from this hypothesis he makes a new deduction, which is also tested by a new experiment; but now, perhaps, he obtains a result, which, although of a positive character, is not what he expected. He has, however, made an advance; he has three facts and an hypothesis to explain two of them. In this case he does not usually abandon his preconceived idea, but modifies it until it includes the new fact. With the hypothesis thus improved, he deduces, it may be in rapid succession, a number of new conclusions, the truth of all of which is borne out by the results of the experiments. The investigator now feels that he is on the right track; that the thread of Dedalus is in his hand, and that he will soon be in the full light of day, but usually the escape from the labyrinth is not so easy. In the height of his successful career, it not unfrequently happens that a result is obtained diametrically opposed to his previous generalization, which conclusively forces upon his mind the conviction that he is still far from attaining his end; that he has not yet seized upon the fundamental principle of the phenomena, which have grown into a class under his hands.

At this stage of the inquiry, his self-esteem is much depressed, he throws aside for a while his apparatus, refers to his library for new suggestions; the subject, however, is not discharged from his mind; it still goes with him, and is perpetually recurring; it is mingled with his dreams, and is seen associated with the every-day occurrences of life, until at length, in some happy moment of inspiration, it may be after refreshing sleep, the truth flashes upon him: he catches a more extended conception of the relations of the phenomena; a more comprehensive hypothesis is suggested, from which he is enabled to deduce in succession a large number of new conclusions to be submitted to the test of experiments. These are all found to yield the expected results, and the generalization which has thus been obtained is more than an hypothesis. It is entitled to the name of a verified theory. The investigator now feels amply rewarded for all his toil, and

is conscious of the pleasure of the self-appreciation which flows from having been initiated into the secrets of nature, and allowed the place not merely of an humble worshipper in the vestibule of the temple of science, but an officiating priest at the altar.

In this sketch which I have given of a successful investigation, it will be observed that several faculties of the mind are called into operation. First, the imagination, which calls forth the forms of things unseen and gives them a local habitation, must be active in presenting to the mind's eye a definite conception of the modes of operation of the forces in nature sufficient to produce the phenomena in question. Second, the logical power must be trained in order to deduce from the assumed premises the conclusions necessary to test the truth of the assumption in the form of an experiment, and again the ingenuity must be taxed to invent the experiment or to bring about the arrangement of apparatus adapted to test the conclusions.

These faculties of the mind may all be much improved and strengthened by practice. The most important requisite, however, to scientific investigations of this character, is a mind well stored with clear conceptions of scientific generalizations and possessed of sagacity in tracing analogies and devising hypotheses.

Without the use of hypotheses or antecedent probabilities, as a general rule no extended series of investigations can be made as to the approximate cause of casual phenomena. They require to be used, however, with great care, lest they become false guides which lead to error rather than to truth.

It is not enough for a physical investigation that we have the simple idea, which may be embodied in a mathematical equation, —we must see clearly, with the mind's eye, the operations in nature, and how the phenomena are produced in accordance with the well-known laws of force and motion.

As an illustration of what I have said, as well as an original scientific communication, I may be allowed to present in this connection an account of some observations in which I have been engaged during the past summer; and which are an extension of the investigation of the phenomena of sound in its application to fog-signals, of the progress of which I have given an account at different times to the Society.

This year, my attention was called to a special phenomenon

observed for several years past on the coast of Maine, which has been classed among those to which the term "abnormal phenomena of sound" is applied. In August, 1873, this was partially examined, and the result published in the Light-House Report for 1874. In order to investigate it further, I associated myself with Gen. J. C. Duane, Engineer of the 1st Light-House District; Commander H. F. Picking, Inspector of the same District; Mr. Edward L. Woodruff, Assist. Engineer of the 3d District; and Mr. Chas. Edwards, Asst. Engineer of the 1st District.

The phenomenon to be investigated was exhibited in connection with the fog-signal at a station called *White-Head*, on the coast of Maine, at the entrance of Penobscot Bay; it was reported as having been frequently observed by the captains of the steamers plying between Boston and New Brunswick, and had also been witnessed on two different occasions by officers of the Light-House Establishment.

The phenomenon, as reported by these authorities, consisted in hearing the sound of a ten-inch whistle distinctly as the station is approached, till within the distance of from four to six miles, then losing it through a space of about three miles, and not hearing it again until within about a quarter of a mile of the instrument, when it suddenly becomes audible almost in its full power.

This phenomenon, according to the statement of the keeper of the light-house station, is noticed whenever the vessel is approaching the station from the southwest, and the wind is in the same direction. It is especially observed during a fog, when the warning of the signal is most wanted, and which in this locality is always accompanied by a wind from the south or southwest.

Our first object was to verify the phenomenon, and for this purpose we steamed to the southwest, directly against the wind, which was blowing at the time at about the velocity of ten miles per hour; this fortunately happened to be the direction of the wind during which the phenomenon was most frequently observed. The whistle was sounded every minute by an automatic arrangement, and the time at which the several blasts were given could be noted from the vessel by the puffs of steam emitted by the whistle. As we increased our distance from the signal, the sound very slightly diminished in loudness, until the distance was about a half mile, when it suddenly ceased to be heard, and continued inaudible for about a mile farther, when it was faintly heard and

continued to increase in loudness until we reached the distance of four miles; at this point it was heard with such clearness that the position of the station could be readily located in the densest fog, but on proceeding still farther in the same direction it gradually diminished and was finally again lost.

As a second experiment we retraced the same line back to the station, and observed the same phenomena in a reversed order. The sound was heard the loudest at a point about four miles from the station, and after that diminished and became inaudible through a space of about two miles, and then suddenly burst forth nearly in full intensity at a distance of a quarter of a mile, and continued loud until the station was reached.

Now, for the investigation of this phenomenon, we may assume provisionally that it is due to a peculiar condition of the atmosphere, either as to heat, pressure, or moisture, or a combination of all of them, which existed at the time in that part of the track of the steamer which may be denominated "the region of silence." But if this were true, such a condition of the atmosphere ought to be indicated by ordinary meteorological instruments. To test this, the temperature of the air was noted through the whole space by an ordinary thermometer, and also its pressure by means of an aneroid barometer, but no variation was observed in these instruments in passing through the air along the path of the vessel.

To complete this series of observations, however, the indications of a delicate hygroscope should have been noted. Unfortunately we were not provided with an instrument of this kind; the fact however that the phenomenon was frequently observed during a fog, or while the air is uniformly saturated with moisture, indicates that the phenomenon is not due to a difference of moisture in the region of silence. Indeed, it is sufficient to remember that a wind was blowing at the rate of ten miles an hour, to be convinced that an isolated portion of air could not remain in a fixed position, even for an instant.

Another hypothesis might be assumed,—that the apparent silence was caused by the transverse reflection, in some way, of sound from the shore, but there was nothing in the configuration of the land which favored such an hypothesis. The only explanation which presented itself was that of the upward refraction of sound, an hypothesis which has been found fertile in new re-

sults in previous investigations of the same subject. To test this, and to ascertain the dependence of the phenomenon on the wind, the position of the focus or the origin of the sound was changed. For this purpose, the whistle of the steamer was sounded, while a portion of the observing force was placed at the station; by this arrangement it was found that while the vessel, in reference to the sound of the signal at the station, passed through a region of silence, the observers at the station, who gave attention to the sound from the steamer, heard no interruption of the signal. This experiment was repeated each way, going to and coming from the station.

From this result it appears that the sound going *with* the wind was heard from every point on its course, while the sound moving against the wind was suddenly lost at a given point and not recovered again until a distance had been traversed by the vessel of more than a mile. This result was in strict conformity with the theory of refraction;—in the case of the sounds travelling against the wind, the upper part of the wave would usually be more retarded than the lower, and consequently the sound wave would be thrown upward, above the head of the observer. At a given altitude this difference of velocity would cease and by the tendency of sound constantly to spread, the sound wave would again reach the earth.

But, to test this still farther and to show that the locality was not an essential condition of the existence of the interval of silence, the experiment was repeated on the opposite side of the station, so that the sound from the fog-signal would move in the direction of the wind. Part of the observers were placed at the station and the other part remained on board the vessel; both instruments were sounded, the one in the intervals of the sounding of the other.

In this case the sound from the fog-signal was continuous to those on board of the vessel through a distance of over four miles, and could probably have been heard many miles farther, but the progress of the steamer in that direction was stopped by the land.

From the report of the observers at the station it appeared that as the vessel passed into the distance but one blast was heard during its whole course. In this case (as in the preceding experiment of sailing to the southwest) the sound moving against the wind was refracted upward, and as the whistle was but six

inches in diameter it did not give sufficient volume to again reach the earth by spreading.

In experiments of previous years the fact has been shown that the sound is heard under certain conditions better when moving against the wind than in the opposite direction. This was notably the case in the experiments made at Sandy Hook in September, 1874, during which a sound from the west was heard *with* the wind about three times as far as a sound from a similar source was heard from the east, or *against* the wind; and, again, the same sound was heard from the west three times as far as from the east after the wind had settled to a calm; and in a third observation the same phenomenon was observed after the wind had increased to a velocity of ten miles an hour from the east. These effects were afterwards shown to be connected with the fact that the wind during the whole day was blowing strongly from the west, and that the apparent changes of the wind were due to currents at the surface, and thus a sufficient explanation was given to the phenomena observed.

It would appear, however, from the investigations of last summer, that the wave of sound, which has been refracted upward, may descend at a greater distance from its origin than even that at which sound moving with the wind can be heard; probably involving a peculiar case of undulating or compound refraction; but this requires further investigation.

Each series of observations gives rise to new questions, and indicates that the subject is one which is rich in new results. Unfortunately, however, the observations can only be made by the aid of steamers; and these, in the Lighthouse service, can only occasionally be employed in the rare intervals of more imperative duties.

In order, however, to collect data for further use in the explication of the phenomena, the Light-keepers at Block Island and Montauk Point (the eastern portion of Long Island) have been directed to blow the fog-signals for an hour on every Monday morning, each noting whether he can hear the sound from the other station; observing, at the same time, the direction of the wind and the apparent motion of the clouds.

From the result of these observations during the year it appears that the clouds give frequent indications of adverse wind currents, and that the number of times that the sound has been

heard against the wind is greater than the number of times it has been heard with the wind; a result which, though unexpected, is not in discordance with previous assumptions.

It will be recollected by the Society that I have, in previous years, mentioned a remarkable phenomenon, which I have denominated the "*ocean echo.*" This has also been observed by the scientific adviser of the Trinity Board, and is considered by him as the key of all the abnormal phenomena of sound observed, and as a special illustration of the truth of his hypothesis that such abnormal phenomena are produced by invisible clouds of flocculent atmosphere. The phenomenon in question consists in a reverberation in the form of an echo from a point in the verge of the horizon to which the axis of a fog-trumpet is directed.

In regard to this I first adopted the provisional hypothesis that this was produced by a reverberation from the crests of the waves of the ocean, but it having been stated that the same phenomenon is exhibited while the sea is smooth, this assumption must be abandoned, or in some way modified to suit the observed facts. To test the hypothesis of the reverberation being due to a reflection from an invisible cloud on the verge of the horizon, the trumpet of the large syren on Block Island was gradually elevated from a horizontal to a vertical position, and while in this position it was sounded at intervals for several days; but in no case was an echo heard from the zenith, but in every instance an echo was returned from the horizon around its whole circumference.

In another experiment with a vertical trumpet at Little Gull Island, a small cloud, from which a few drops of rain fell on the area of the base of the Lighthouse, passed directly across the zenith, and during this passage no echo was observed from the cloud, although the trumpet directed toward it was sounded several times in succession.

Again, in order to obtain additional facts in regard to the nature of this echo, observation was made from a vessel, by steaming out directly as if into the region of the echo; *i. e.*, in the direction of that point in the horizon from which the echo appeared to emanate.

In this case the loudness of the echo appeared, as we advanced, to gradually diminish, and to spread itself through a much longer arc of the horizon, while the duration of the echo increased in time.

It would follow from this experiment that the echo is not a reflection from a definite surface, since it would then increase in loudness as the surface is approached; but a series of rebounds from points at various distances.

Another fact of great importance in determining the nature of the echo is that derived from the observations of the keeper at Block Island; he has recorded during the observations of a year, every Monday, the length of the continuance of the echo, the state of the weather, the direction of the wind, and the other meteorological data. From which it is found that the echo is always heard with the sound of the syren during a wind in any direction, and of all intensities; but with less duration after the original blast, during the occurrence of a very high wind, than in calmer weather; and, above all, that it is heard equally well during a dense fog, when evidently the air must be homogeneous and saturated in every part with vapor.

From these facts it appears to me conclusive that the reverberation, constituting the *ocean echo*, cannot be due to invisible clouds. The only hypothesis which suggests itself to my mind as a basis of further investigating this subject, is that in the spread or divergency of the sound the direction of the impulse turns through an angle of a little more than 90°, so as to meet the surface even of the smooth ocean in a direction by which it would be reflected to the ear of the observer, making the angle of reflection equal to the angle of incidence; although from the gradual dispersion of sound-beams, the precise equality of these angles is obviously not very important to the result.

On returning from this excursion by the N. Y. Western Railway to the Hudson River at Troy, opportunity was taken to make some observations on the action of sound in the Hoósac tunnel, through which I passed, accompanied by Mr. E. L. Woodruff, on the afternoon of September 7th. Resting at East Windsor near the western outlet, I spent a considerable part of the following day in making an examination of the work. Mr. W. P. Granger, the chief engineer, and Mr. A. W. Locke, his principal assistant, very courteously furnished a hand-car, and cordially proffered every facility for making any desired investigations. This tunnel, as is familiar to most of those present, is nearly five miles long, rising by an easy grade of 26.4 feet to the mile from either mouth to about the middle of the tunnel, where it

opens into a vertical ventilating shaft through the rock, of upwards of a thousand feet in height. The top of this shaft opens between two ridges of the Hoosac mountain, which rise respectively some 400 and 700 feet higher. From the middle of the tunnel when entirely clear of smoke, the distant opening at either end appears as a faint star. The darkness seems oppressive; and when a train is passing through, the air becomes so thickly clouded that the glare of torches cannot be seen at more than a dozen feet distance.

It had been constantly observed by those employed in the tunnel, that during the approach of a locomotive at no great distance, and a few minutes afterward, the sound of the engine was very much deadened and obstructed; so much so indeed, as to imperil the workmen engaged in lining the top of the tunnel with a brick arch, who frequently failed to hear the locomotive until it was close upon them. This obscuration of sound was not unnaturally attributed to the dense clouds of smoke constantly emitted by the locomotive: but this explanation can hardly be accepted as the true one, nor the condition noted, as constituting even an appreciable cause of such acoustic opacity. When we reflect that a puff of exhaust-steam at high temperature is ejected at about every four feet of rail traversed by the driving wheels, it is not difficult to realize that in an atmosphere so systematically made heterogeneous, there must be a very great amount of dispersion and absorption of sound waves struggling through such a medium. This has been well illustrated by the striking experiments of the distinguished physicist of the Royal Institution. A very simple method of confirming this explanation, and of eliminating entirely the effect of the smoke, would be the employment of locomotive engines driven by the combustion of coke or of charcoal. This experimental determination of the question did not occur to me till after we had left the tunnel; but on suggesting it to Mr. A. W. Locke, the assistant engineer in charge, he very obligingly undertook the conduct of such an experiment at the earliest convenient opportunity. The result has not yet transpired.

When the tunnel was entirely clear, and a gentle current of air flowing down the central ventilating shaft and out at the two ends (as is usual in the summer season when the external temperature is higher than the internal), it was observed that a prolonged but irregular echo followed any loud noise, such as the

sudden shutting down of the lid of a tool-chest. The unequal or somewhat intermittent character of the echo appeared to result from the irregular surface of the rock forming the walls of the tube. A somewhat similar echo is sometimes returned from the dense foliage of trees. It is proper to add that a very perceptible echo was heard from the portion of the tunnel lined with brick. The effect could in neither case be ascribed to any invisible "flocculence," as the air must have been in a very homogeneous condition.

Inasmuch as in such observations the waves of sound are reflected back to the ear from points at a considerable distance from their origin, this being especially true of the ocean-echoes, we are liable to be seriously misled if we rely too confidently on the experiments of the laboratory; and form hasty generalizations from apparent analogies, without carefully considering *all* the meteorological conditions by which the rays of sound may be deflected, distorted, and diverged. It is now well established by numerous observations and experiments—made independently on both sides of the Atlantic, that the lines of acoustic propagation (conveniently called sound-beams) which are sensibly very rectilinear for the distance of a hundred or two hundred feet, and which are thus obedient to the katoptric and dioptric laws of precise focal convergence, by means of solid mirrors and of gaseous lenses, are yet at the distance of a few miles so strangely contorted and aberrant, as seemingly to contradict all the analogies suggested by our experience with the rays of light. It is the accumulation of comparatively slight divergencies continued through many thousands of yards, whether under the influence of constant conditions, or of changing and reversed conditions, which produces such marked anomalies at the distance of five or of ten miles, and which makes their investigation as laborious as it is instructive and important. And not until we have mastered all the conditions affecting the transmission of sound throughout its entire sensible range, and have thus become enabled to predict its true course, and to announce its varying limits of audibility at the earth's surface, under given circumstances, can we be said to have perfected the theory of this most interesting and indispensable agent of communication.

Remarks were made by Mr. ANTISELL.

Mr. GARRICK MALLERY commenced a paper on

SOME COMMON ERRORS RESPECTING THE NORTH AMERICAN INDIANS.

Mr. ALVORD remarked on the disagreement between archæologists and others respecting the origin of the American Indians.

132D MEETING. DECEMBER 8, 1877.

Vice-President HILGARD in the Chair.

Fifty-six members and visitors present.

Mr. MALLERY continued his paper on

SOME COMMON ERRORS RESPECTING THE NORTH AMERICAN INDIANS.

(ABSTRACT.)

The traveller Catlin announced in his well-known Letters, dated 1839—"the Indians of North America are copper colored . . . were sixteen millions, and sent that number of daily prayers to the Great Spirit, and thanks for his goodness and protection."

The Sioux commission of 1876 urged, as an argument for political favor, that "the Indian is one of the few savage men who clearly recognize the existence of a Great Spirit."

De Tocqueville remarked of the American tribes—"there is no instance on record of so rapid a destruction."

The joint special committee of the two houses of Congress, in 1867, reported—"the Indians everywhere, with the exception of the tribes within the Indian Territory, are rapidly decreasing in numbers."

McKenney and Hall in their magnificent work, published in 1844, declare "all the tribes with which we are acquainted are in a state of rapid and progressive diminution"

One of the latest ethnological writers, Mr. Hubert H. Bancroft, states in his "Native Races," with philosophic emphasis, "the intercourse of civilized with savage people results in the disappearance of civilization or the extinction of the barbaric race," and bewails "all the millions of native Americans who have perished under the withering influence of European civilization."

These quotations exhibit some of the most important current errors regarding our aborigines. As read, the statements probably would receive general assent, but they are all seriously incorrect.

Mr. Catlin's designation of the color of copper for the Ameri-

can race was by no means original, having been used by many scientists (who never saw an Indian), in their classifications, sometimes under the less distinctive term red; but the Indians are neither red nor copper colored, having been first styled so from the universal use, for personal adornment, by those their discoverers first met, of the ochre found in their soil, whereby the skin remained stained long after the artificial hue had ceased to be fresh, and, as the brighter imported pigment became accessible to and greatly preferred by them, the hue of the ruddy metal for their description might well be amended into that of vermilion. It is true that, imitating the designation of their discoverers, the eastern Indians have called themselves "red men," but bands near the Rocky Mountains, who during the present century first met explorers of European descent, spontaneously styled the latter red, to mark the contrast of the sun-blistered faces of their visitors with their own darker skin. Their real prevailing color is brown, though with many various shades, some of which are not distinguishable from those found in other parts of the world, especially Asia, and there is no more propriety in styling them red than it would have been for Cæsar to have called the ancient Britons blue, when he noticed that they universally stained their bodies with woad. Without entering upon a too vast field of discussion, it may be indicated that the attempt to segregate our Indians from the rest of the world, by a color classification, has been even less successful in distinct results than that of craniologists.

To deny that the Indians believed in and worshipped an overruling power (which we have commonly called the Great Spirit or Manito), seems to be an iconoclastic assault upon a favorite field of religious illustration, perhaps tending to impair some, however superfluous, theological arguments. A better acquaintance, however, with our continent's traditions, and particularly with the etymology of its languages, shows that myths have been misunderstood, and the epithets of divinity mistranslated, when they have conveyed either the idea of monotheism, or of any personal and definite God with the attributes given by us to that word or the Latin *Deus*. The more learned missionaries are now not only agreed that a general creator or upholder never existed in aboriginal cosmogony, but that the much simpler belief in a single superhuman great chief or ruler is a modern graft.

The Jesuits in their relations confess that no one immaterial god was recognized by the Algonkins, the title Manito having been introduced by themselves in a personal sense, and that of the head Iroquois deity "Neo," or "Hawaneu," is asserted on linguistic grounds to be a mere corruption of the French "*Dieu*" and "*le bon Dieu.*" One of the last claimants for a native god of causation was a missionary to the Cherokees, who boasted that he found the word of that language for "maker" used as a divine title, but, on being cross-examined by Prof. J. W. POWELL, he was forced to admit that the word did not mean "maker" in

the abstract (which the genius of no Indian tongue could express), and, from its incorporated pronominal particle, could not, as used by the Cherokee, signify "*my* maker," but *his*, *i. e.* the white man's maker, thus showing only the readiness with which the latter was admitted into America's elastic Pantheon. Doubtless in councils and other intercourse with Christians, Indian speakers employed the words *Manito*, *Taku Wakan*, and the like, in a sense acceptable to the known prejudices of their interlocutors, but that was through courtesy and policy, much as the strictest Protestant would once have found it convenient if not necessary, when at Rome, to speak respectfully of the Pope. The adoption of expressions as well as of ideas which were understood to be agreeable to or expected by the whites, is well illustrated in the use by western Indians of the terms "squaw" and "papoose," which are not in their languages, but are mere corruptions from the Algonkin. As all travellers insisted upon those words to signify woman and child, the tribes, as successively met, complied, with the result of a general belief that they were common to the several native dialects, which is no more true than if the English terms had been impressed upon them instead of those equally foreign.

The sixty-three linguistic families on this continent north of Mexico, some differing from each other in speech more widely than the Latin from the Teutonic nations, and even rivalling in degree the distinction between Indo-European and Semitic dialects, naturally present myths greatly diverse, but agree with marked unanimity in acknowledging no Supreme God, and in dividing all supra-human power among many personages to be propitiated, appeased, and utilized. It is true that they did not reach the advanced culture in which the Greek, Scandinavian, and other inheriters of earlier orient folk-lore produced the distinct figures of Zeus, Thor, Phœbus, Astarte, and Boreas. Scholars have, however, lately traced these classic personifications of sky, thunder, sun, moon, and wind, to their rude Aryan or Hamitic originals, which differ but slightly, save in the tincture of racial idiosyncrasy and habitat, from those of our Indians, while we sometimes strike curious native parallels to the serpent of Midgard and tortoise of Vishnu, the bridge of Al Sirat and the Elysian Fields, the labors of Herakles and doom of Sisyphus, Titanic wars and Cyclopean struggles.

In the infancy of all races appears what has, with doubtful propriety as applied to that stage of development, been styled nature worship, being at first merely an attempt to account for surrounding phenomena. There could have been no clear conception of the supernatural, because there was yet none of natural order—no miracle when there was no law to be suspended or changed. The human mind in its early development tried to explain the unknown by classification with what was already known, much as a scientific law is now formulated only after

proper relegation of ascertained facts to a category of similars, though among the latter there must be eventual discrimination between mere homology and true analogy. The savage only understood human feelings and capacities, and so peopled his philosophy with imagined actors to perform every operation of nature beyond his own powers. Thus many forms of being, motion, and action, became the work of personages with man's volition, and differing chiefly from him by greater intelligence, strength, and size. The polytheism naturally resulting was not so disgraceful to humanity as has been claimed, for the methods of modern science have only improved upon the barbaric or Archaic efforts through greater experience and more careful restrictive tests to guard against false conclusions from association of ideas, and tempting first impressions of cause and consequence. The sequence in mental progress is, 1st, Mythology; 2d, Metaphysics; 3d, Positive Philosophy.

It would then have been indeed strange, if the cosmogony and religion of our native tribes had contrasted greatly with those of our own ancestors of the stone age to which, of the old world's periods, they relatively belong. In fact there is no such contrast. The Indian filled nature with spirits only in the sense of explanation before mentioned, some one of his Anthropomorphic or Zoömorphic conceptions to answer the pressing conundrums of *how* and *why*, ever personally accomplishing or acting in all phenomena whether spasmodic, continuous, or intermittent, with no relation to any general order, rule, or Providence. We now account for a thunder-storm by known rules of evaporation and condensation. The Indian was driven to invent a monstrous disturbing and overshadowing eagle, and he both explained and symbolized the howling wind by the howling wolf. The Dakota sees a Wakan not only in every unusual occurrence, but in each remarkable rock and noisy cataract, and recognizes its divinity by a tribute of tobacco. Among the Iroquois, their staples, corn, beans, and squashes, being planted and tended, were collectively the gifts and constant care of the "Three Supporting Sisters"—"De-o-há-ko"—but any one of the secular oaks and sequoias, the growth of which is not observed, may have in the regions where they are found its individual numen. The red tuft of the woodpecker, the blindness of the mole, the forked tongue of the snake, and the spark from the flint, each had its storied cause in the adventures of ancestors and daimons.

It is not correct to call our Indian a Zoöloter, except in so far that his intense study of the habits of animals has individualized and personified their special characteristics, and that, taught by fasting visions, he generally adopts some bird or beast in mutual relation of protection and respect, though not often strictly with worship. He had no special cult of a living animal, such for instance as of the bull Apis, but deified its mystical progenitor or prototype. Michabo, the Great Hare, was to the Algonkins

their own ancestor, founder of their religious rites, and ruler of the weather, while the coyote was the parent and benefactor of the Karoks and other Californians. The rattlesnake was a general "grandfather," and, though greatly feared, was, it is said, never intentionally killed. Some authors have asserted that fetichism is not found in American religious, but that would only be true with a narrow definition of fetichism, which is but one form of animism, and should include all attribution of voluntary power to inanimate objects, not, as are idols, representative or symbolic, which is as prominent a feature in Indian as in African mythology. Even the most repulsive fetichistic details survive in what has, foolishly enough, been translated "medicine," embracing charms and amulets, the fossil tooth carried by the Assiniboin, the tail feathers of the chapparal cock sacred among the Cheyennes, stones with vivid spots, colored earth or sand, bones and ashes of animals, birds and reptiles, deposited in bags with ceremonial chants and dances, and possessing as used deadly or saving virtues. We find here, in short, with new faces, most of the antique foes of Christianity, *e. g.* Antientism, hero and astral worship—sometimes mingled, as when an Iroquois tribe revered Ioskeha, born of a virgin daughter of the moon, as its father and bestower of fire—metempsychosis of man and beast, apparitions and sorcery, oracle and disease-possession by good and bad spirits, and the eastern psychopomp has its analogue in the dog slain, or the bird loosed at the grave. Our Indians, so long secluded and delayed in their sociological culture, were on their discovery no better and no worse, religiously, than the population of what it is the fashion to call *Juventus mundi*. Referring then to Mr. Catlin's pathetic lament, if all the members of their polytheism, or rather polydaimonism, had been addressed on any day by each native inhabitant of the continent, the list of prayers would have far exceeded his sixteen millions, but the multiplication would have been produced by the census of the divinities as a factor quite as important as that of their worshippers.

This leads us to consider the common belief that the native population on the arrival of the first colonists was very large, has been and still is rapidly becoming extinct, and that the cause of that extinction is an inherent characteristic or defect of the race rendering impossible its civilization or even existence with civilized environments.

(*The part of the paper under this head will be published in the Proceedings of the American Association for the Advancement of Science. Nashville Meeting.* 1877.)

The conclusions reached are, that the pre-Columbian population of the territory occupied by the United States has been wildly overestimated; that, while many of its component bodies have

been diminished or been destroyed by oppression and violence, their loss has been in large part compensated by gain among others, that the "blight" and "withering," or *feræ naturæ* theory is proved to be absurdly false, and that, though some temporary retrogradation must always be expected among individual tribes, at the crises of their transition from savagery or barbarism to more civilized habits, yet now the number of our Indians is on the increase, and will naturally so continue unless repressed by causes not inherent to civilization, but to criminal misgovernment, until their final absorption into the wondrous amalgam of all earth's peoples which the destiny of this country may possibly effect. Neither from views of their physiological, religious, or sociological characteristics, should they be regarded as an exceptional or abnormal part of the human race, or so treated in our national policy. Only those legislators and officials, who are prepared to encourage downright murder, can neglect their duty under the Satanic consolation of the convenient extinction doctrine. With continued injustice more Sitting Bulls and chief Josephs, driven into the last refuge of despair, will require expenditure of blood and treasure which simple truth and honesty would prevent, while judicious and consistent treatment would preserve, reclaim, and elevate a race entrusted to our national honor, which may with no interminable delay become a valuable element in our motley community.

Mr. Powell spoke of the greater permanency of the larger confederacies of the North American Indians; also of the knowledge of medicine attributed to them, and their having poisoned arrows, as fallacious. They used charms and charmed arrows. The popular idea of their languages being meagre, and requiring facial expression to convey their meaning, is incorrect.

Mr. Woodward remarked on the danger of generalizing from one country to another. The South American Indians have poisoned arrows.

Mr. Mason spoke of the arrows from blowpipes of the South American Indians, and of the "black drink" used by them, which is a purgative and an emetic: also of the immense ancient population in the Valley of the Mississippi, as evidenced by mounds, monuments, and remains. He further remarked that the truth as to the number of Indians in North America at the time of its discovery by Europeans was probably between the extreme estimates at first and the subsequent skeptical doubts; but there was very little ground to stand on, and admitting widely different speculations.

Mr. POWELL referred to the mound as being a burial-place for many generations, and the large number of remains in any of them as not indicating a large contemporaneous population. He also spoke of the Pueblos as moving southward to newer, larger, and better structures, and that those abandoned should not be regarded as indications of extermination.

Remarks were also made by Mr. WHITE and Mr. ALVORD.

133D MEETING. DECEMBER 22, 1877.

Vice-President TAYLOR in the Chair.

Thirty-four members and visitors present.

The election of Dr. DAVID LOWE HUNTINGDON, U. S. A., and Mr. HENRY ROBINSON SEARLE, as members of the Society, was announced.

Mr. C. A. WHITE made a communication on

SOME PHASES OF THE EVOLUTIONAL HISTORY OF THE NORTH AMERICAN UNIONIDÆ.

Remarks were made by Mr. GILL; Mr. ANTISELL, who objected to the idea of propagation from salt to fresh water; Mr. GILBERT, on the accommodation of fresh-water types to brackish water, instancing fresh-water shells in the brackish water about Great Salt Lake; and by Mr. GILL, who referred to the existence of sharks and other salt-water fishes in the fresh water of Lake Nicaragua.

Mr. ASAPH HALL read a paper on

THE POSITION OF THE CENTRE OF GRAVITY OF THE APPARENT DISK OF A PLANET,

giving the method he adopted in measuring the distances of the satellites of Mars from the limbs of the primary, and reducing them to the centre.

(This paper appears in the Monthly Notices of the Royal Astronomical Society for January, 1878.)

Mr. THEODORE GILL made* a communication on

A NEW SPECIES OF CHIMÆRA FOUND IN AMERICAN WATERS.

One of the most unexpected discoveries recently made in American ichthyology is that of a species of the genus *chimæra*, of which a specimen has lately been sent to the Smithsonian Institution. It was caught southeast of the La Have bank, in lat. 42° 40′ N., lon. 63° 23′ W., at a depth of 350 fathoms, with a bait of halibut. An attentive comparison of the specimen with individuals of the European *chimæra monstrosa*, renders it evident that it does not belong to that species, but is an entirely distinct specific form. It may be named *chimæra plumbea*, and diagnosed as follows:—

Chimæra plumbea.

A *chimæra* with the snout acutely produced; the ante-orbital flexure of the suborbital line extending little above the level of the inferior margin of the orbit; the dorsals close together; the dorsal spine with its anterior surface rounded; the ventrals triangular and pointed; the pectorals extending to the outer axil of the ventrals; and the color uniformly plumbeous.

By these characters the species is readily separable from the *chimæra monstrosa* and other species of the genus.

Mr. J. W. POWELL made remarks on

POISONS AMONG THE NORTH AMERICAN INDIANS,

regarding the tale of their dipping arrows in deer's liver poisoned by the bites of rattlesnakes as a myth. He spoke also of arts among the South American Indians as derived from a higher state of civilization than those found among the North American Indians.

Mr. WOODWARD quoted Dr. HOFFMAN, U. S. A., as having seen Indians poisoning their arrows in the way described, and as having been informed by some of them that they carried different arrows for destroying animals and their enemies.

He also, on the authority of Dr. COUES, U. S. A., spoke of poisoned wounds from arrows of the North American Indians.

* Received by the Secretary of the meeting, December 17.

Mr. POWELL remarked that they dipped their arrows in blood and flesh.

Mr. ANTISELL remarked that the Apaches carried poisoned arrows, obtaining the poison from more southern tribes, who derived it from the rattlesnakes.

Remarks were also made by Mr. FARQUHAR and Mr. GILL.

134TH MEETING. JANUARY 5, 1878.

Vice-President WELLING in the Chair.

Thirty-eight members and visitors present.

Mr. ELLIOTT COUES, after a few preliminary remarks on the evidence of

"THE USE OF POISONED ARROWS BY NORTH AMERICAN INDIANS,"

and his own experience and that of others with wounds produced by them, read a paper on the subject by Dr. HOFFMAN, Surgeon U. S. Army.

Mr. GALE made a communication on

"THE CLIMATE OF PLANTS,"

maintaining that each plant has a range of climate within which it thrives and flourishes, and outside of which is dwarfed or disappears, illustrating by familiar examples.

Mr. ANTISELL gave instances of the disappearance of trees in some localities not attributable to climate, some of which Liebig ascribed to changes of soil; for others no satisfactory reasons had been assigned. He instanced pines on the coast of California, where the denuded soil and greater exposure to winds were unfavorable to the growth of young plants; and the bamboo on the northern Island of Japan, which, stinted near the shore, becomes more luxuriant as you go inland, and grows well on the

top and sides of hills, but may flourish less below; vitality diminishing by exposure to winds.

Mr. G. K. GILBERT made a communication on

A PROPOSED NEW LEVELLING INSTRUMENT

for scientific uses. To the usual combination of spirit level and telescope, he proposed to add a graduated rod, so attached that when the rod was adjusted to verticality the optical axis of the telescope would be horizontal. The rod would extend as far below the telescope as it did above, and the tripod head would be pierced to admit the passage of the rod. In use the instruments would be handled in pairs, each being in turn carried forward past the station of the other. Each reading would be reciprocal, the height of the higher telescope being read on the upper half of the rod of the lower instrument, and the height of the lower telescope on the lower half of the upper rod. The mean of the reciprocal readings would need no correction for curvature of the earth or for normal refraction, and an inspection of the sum of the reciprocal readings would lead to the detection of errors arising from mal-adjustment, from mistakes, or from unfavorable conditions of atmosphere or ground surface. An important use of the instrument would be for the investigation of the conditions affecting the precision of levelling, so as to determine the most favorable weather, the most favorable hours, and the most favorable character of ground surface.

Remarks were made by Messrs. COFFIN, HALL, and DALL, the latter objecting to the instability of such an instrument even in light winds.

135TH MEETING. JANUARY 19, 1878.

Vice-President WELLING in the Chair.

Fifty-seven members and visitors present.

The election of Mr. ARNOLD BURGESS JOHNSON as a member of the Society was announced.

Mr. J. E. HILGARD exhibited and described an

"OPTICAL SALINOMETER,"

consisting of a prism to contain a saline solution, and a telescope and micrometer for measuring changes in the index of refraction, as the strength of the solution is changed. He claimed for it great delicacy and adaptation to use at sea, as it would not be affected by the motion of the vessel.

Remarks were made by Messrs. DUTTON and ANTISELL.

Mr. J. M. TONER exhibited a malformed dog, only a few days old, which Mr. WOODWARD described as a monstrosity in excess; the forward portion being single, while from the waist down it was a pair of twins. The middle leg, however, comprised a right and left leg united under the same integument, and he supposed that the tail would also, on dissection, be found double. He then referred to the case of a Portuguese man, well known to the medical profession, and to a recent paper by RAUBER.*

Prof. JAMES D. BUTLER, of Madison, Wisconsin, made a communication on

PREHISTORIC COPPER,

exhibiting a variety of copper implements found in Wisconsin, a few feet below the surface, and describing their uses and the localities from which they were obtained.

Remarks were made by Messrs. WHITE and GILBERT, chiefly on the copper drifts in Iowa and Missouri, derived from the copper region of Lake Superior; by Mr. DALL on the copper implements made and used at the present time in Alaska; by Mr. POWELL on similar implements of stone made and used by other tribes in North America, and by Mr. MASON on the many mounds in the region described by Mr. BUTLER.

* Die Theorie der excessiven Monstra. VIRCHOW's *Archiv*, Bd. 71, 1877, S. 137.

136TH MEETING. FEBRUARY 2, 1878.

Vice-President TAYLOR in the Chair.

Thirty-eight members and visitors present.

The election of Mr. JACOB KENDRICK UPTON as a member of the Society was announced.

Mr. ASAPH HALL communicated

THE RESULTS OF HIS SEARCH FOR SATELLITES OF MARS,

and the eventual discovery of the two now recognized, and proposed for the inner satellite the name of Φόβος, and for the outer the name of Δεῖμος. He considered that the great velocity of the revolution of the former around Mars constituted an objection to the nebular hypothesis as propounded by La Place.

(*Mr. Hall's paper will appear in the next volume of the Observations of the Washington Observatory.*)

The subject was discussed especially in its relations to the nebular hypothesis by Messrs. ABBE, TAYLOR, DOOLITTLE, GILBERT, and NEWCOMB.

Mr. LESTER WARD commenced a communication on

THE NATURAL SYSTEM OF PLANTS.

137TH MEETING. FEBRUARY 16, 1878.

Vice-President WELLING in the Chair.

Thirty-eight members and visitors present.

Mr. PARKER, for a Committee appointed at the last meeting reported the following resolutions, which were unanimously adopted :—

The Philosophical Society of Washington having heard with unusual interest the communication of Professor ASAPH HALL, U. S. N., giving an account of his discovery of two satellites of Mars, therefore,

Resolved, That this important discovery by one of its members constitutes an event which not only the Society appreciates, but

which will also be regarded with interest by the present age, so distinguished for its steady advance into the realms of the unknown.

While the members of the Society congratulate Professor HALL upon the success which has crowned his persevering efforts, and admire the modesty which characterizes his account of them, they feel proud to claim him as one of their number.

As every one who makes an important discovery either in the arts or sciences, beneficial to mankind, stamps a coin that will transmit his name with honor to future ages, so in time to come will be perpetuated the name of the distinguished discoverer of "Δείμος" and "Φόβος".

Resolved, That a copy of the above resolution, engrossed and signed by the President and Secretaries, be presented to Professor HALL, with the request that he will permit his paper to be published in the Bulletin of the Society.

Mr. LESTER WARD continued a communication on

THE NATURAL SYSTEM OF PLANTS.

Mr. ANTISELL made remarks on the proper basis of classification; Mr. WHITE on fossil plants; and Mr. GILL on the cautiousness with which resemblances should be used, though they may be taken as a guide in first examinations, giving instances of strong resemblances in animals of different orders, and of superficial resemblance and different anatomical structure.

Mr. G. K. GILBERT made a communication on

THE RECENT HISTORY OF THE GREAT SALT LAKE,

giving the evidence, obtained while engaged in investigating the agricultural resources of Utah, of an increase in the flow of the streams, and rise and increased area of the lake itself. There were also annual fluctuations caused by the melting snows in spring and the heat in summer. He also discussed the causes to which the progressive rise has been attributed, viz., volcanic action, change of climate, agency of man. He spoke also of similar increase in the streams and lakes of Colorado.

Remarks were made by Mr. HAYDEN and Mr. WHITE.

138TH MEETING. MARCH 2, 1878.

Vice-President HILGARD in the Chair.

Fifty-one members and visitors present.

Mr. M. H. DOOLITTLE made a communication on

THE NEBULAR HYPOTHESIS AND THE INNER MOON OF MARS.

(*See page* 190.)

Mr. W. B. TAYLOR remarked that the suggestion of meteoric matter was worthy of consideration; yet, although a *vera causa*, it was an insufficient one to produce any sensible effect on the motions of any of the planets, except perhaps on Mercury. He also spoke of the inner ring of Saturn as revolving more rapidly than the planet; also as approaching the planet; and the dusky ring as also approaching and becoming brighter.

Mr. HALL questioned this statement, and remarked that micrometer measurements of these rings were somewhat unsatisfactory; but his own accorded with those of Herschel. Drawings are not trustworthy for determining changes. He contended that the motion of the inner satellite of Mars was not in accordance with La Place's nebular hypothesis. He also remarked that the motions of Encke's comet in some periods indicated a resisting medium; in others not.

The discussion of Mr. GILBERT's communication on the recent history of Great Salt Lake, presented at the last meeting, was resumed: Mr. ALVORD read a letter on the increase of rain-fall in that region, and further remarks were made by Messrs. GILBERT, WHITE, and DUTTON.

139TH MEETING. MARCH 16, 1878.

Vice-President TAYLOR in the Chair.

Thirty-eight members and visitors present.

The election of Dr. ALEXANDER Y. P. GARNETT as a member of the Society was announced.

Mr. J. W. POWELL made a communication on

THE LANDS OF THE ARID REGION OF THE UNITED STATES,

and expressed the belief that of the whole area of Utah, $2\frac{8}{10}$ per cent. was arable, and 23 per cent. was of use for the growth of timber; and of the last, almost half was covered with standing timber more or less scattered, and the remaining portion had been devastated by fires or otherwise.

The subject was discussed by Messrs. GILBERT, WHITE, COUES, and WARD.

Mr. W. B. TAYLOR supplemented his remarks of the last meeting by a further discussion of the analogy of the Rings of Saturn, as to the probable differences of the outer and inner elements, with the satellites of Mars.

Mr. HALL stated that he had consulted the memoirs of Herschel on the Rings of Saturn, and according to that astronomer's observations there was no appreciable difference in the periods of rotation of the outer and inner portions.

140TH MEETING. MARCH 30, 1878.

Vice-President TAYLOR in the Chair.

Fifty-six members and visitors present.

The death of Mr. FERDINAND KAMPFF, a member of the Society, was announced.

Mr. ELISHA FOOTE read a paper on

THE CAUSES OF ELECTRICAL DEVELOPMENTS IN THUNDER STORMS,

in which he attributed the electrical phenomena to the condensation of vapor, and in support of his views, detailed experiments which he had made.

The paper was discussed by Messrs. ABBE, ANTISELL, E. B. ELLIOTT, F. W. CLARK, and W. B. TAYLOR.

Mr. C. A. WHITE made a communication on

ASYMMETRY IN THE FORM OF THE HUMAN CRANIUM,

and exhibited in illustration the imprints of a hatter's craniometer made from 200 heads.

The subject was discussed from different points of view by Messrs. POWELL, GILBERT, WOODWARD, GILL, OTIS, E. B. ELLIOTT, and HALL.

Mr. M. H. DOOLITTLE made a communication, supplementary to that of the meeting of March 2, on

THE INFLUENCE OF AEROLITES ON PLANETARY MOTION,

and stated that Prof. Winchell independently and slightly previously to himself had arrived at the similar conclusion in part.

(Abstract given below.)

Prof. J. LAWRENCE SMITH, of Louisville, Ky., by invitation, spoke on the subject, and expressed his dissent from the view that aerolites, strictly so called, could have exercised any appreciable influence on the mass of the earth.

141ST MEETING. APRIL 13, 1878.

Vice-President WELLING in the Chair.

Fifty-three members and visitors present.

Mr. M. H. DOOLITTLE continued his communication on

AEROLITHIC DISTURBANCE OF PLANETARY MOTIONS.

(ABSTRACT of communications on March 2, 30, and April 13.)

The term "aerolite" is employed to denote not only meteoric stones, but also shooting stars, "cosmical dust," and whatever matter is gathered by the sun and planets from interplanetary and interstellar space.

The periodic time of a planet is given by the formula—

$$\textit{Periodic time}^2 = \frac{\textit{mean distance}^3 \times \textit{constant}}{\textit{mass of sun} + \textit{mass of planet.}}$$

By aerolithic increase of the mass of the sun, and consequent augmentation of centripetal force, the planet is drawn inward, and its mean distance diminished. The periodic time is therefore diminished, both by decreasing the numerator and increasing the denominator of the above fraction.

Aerolithic increase of the mass of a planet has a similar effect; and the mean distance is generally still further diminished by resistance to the motion of the planet. It has been ascertained by observation that the earth meets more aerolites than overtake it; and it may reasonably be assumed that the absolute motions of all the aerolites encountered by a planet nearly neutralize each other, and that the resultant effect on the motion of the planet is nearly the same as if the entire addition to its mass had previously been absolutely at rest. In a similar manner these additions to the mass of the sun diminish the velocity of its revolution around its own axis.

Similar reasoning is evidently applicable to the case of planet and satellite. In all these ways the relative velocity of a satellite in its revolution around its primary is increased as compared with the velocity of the axial revolution of the latter.

From Dr. von Asten's computations, it appears that Encke's comet encounters an irregular resistance, sometimes returning to perihelion as if it had encountered no resistance whatever. The uniform resistance of the luminiferous ether or of a cosmical atmosphere seems therefore to be utterly inappreciable; and it may be inferred that if there is any such resistance, it is of small importance as compared with aerolithic disturbance.

If the inner moon of Mars was formed from that planet, in accordance with Laplace's nebular hypothesis, and has since been brought to its present position solely or principally by aerolithic action, it is evident that a very large addition has been made in that manner to its original mass. It is suggested that the eccentricity of the planetary orbits, and the want of coincidence between the planes of the orbits and equators of the planets and the plane of the solar equator, is perhaps better explained by the action of aerolites than by differences of temperature and density, as supposed by Laplace. These considerations also indicate immense additions to the original planetary masses.

It is certain that unmodified meteoric stones form but a very small part of the accessible matter of the earth; and it may thence be inferred that by far the greater part of this aerolithic acquisition was made during an immensely long cosmological period before the earth solidified into a record-keeping condition. But it is hardly safe to assume that the chemical constitution of the meteoric stones resembles that of shooting stars in general; and

the meteoric stones may form but a small part of the entire aggregate of the earth's aerolithic acquisitions. There is also much reason to believe that aerolithic action was vastly more efficient in the remote past than at present. The quantity of aerolithic matter capable of furnishing such acquisitions must have been perpetually diminishing, unless it is infinite; and if it is scattered through infinite space, it is reasonable to suppose that the number and quantity of the stars is also infinite. Unless this infinity is of a lower order than that which represents the quantity of aerolithic matter, the latter must have been perpetually in process of exhaustion.

Remarks were made by Mr. HARKNESS on the similarity of the spectra of comets and heated meteoric stones; by Mr. POWELL on the want of any geological indications of meteoric accumulations; by Mr. HALL on the marked eccentricity of the orbit of the inner satellite of Mars, while the orbit of the outer satellite is nearly circular; and by Mr. GILBERT and Mr. ALVORD.

Mr. E. B. ELLIOTT made a communication on a proposed instrument which he called

THE TELEPHOTE,

in which, alluding to the fact that the public mind at the present time was much exercised on the subject of the telephone, an instrument for the transmission of sound to a distance, he suggested that by the passage of intermittent electrical currents through rarefied vapor by means of the apparatus known as the Gassiot or Geissler tubes, the vibrations of the plate may be made visible, and thus might be applied with advantage for the benefit of deaf mutes, conveying an intelligent impression of music, and perhaps speech, to the eye.

Mr. THOMAS ANTISELL made a communication on

TEMPERATURES OF THE PACIFIC OCEAN,

exhibiting charts by which were represented for particular portions of each year the temperature of the water as noted during fifty trips of one of the Pacific mail steamers between San Francisco and Yokohama. On the Asiatic side the minimum temperature was 60° in March; the maximum 84° in July. On the American side the minimum and maximum in the same months

were 53° and 73° respectively; a temperature 7° to 14° lower than on the Asiatic side.

He also pointed out the courses of the Equatorial and Asiatic currents of the Pacific Ocean, and the indications of a current of cooler water flowing southward along the western coast of North America.

Mr. DALL made remarks on currents and temperatures in the Sea of Oskotch and Behring's Bay and Straits, calling attention to the shoalness of the latter.

142D MEETING. APRIL 27, 1878.

Vice-President TAYLOR in the Chair.

Twenty members and visitors present.

Mr. EDGAR FRISBY made a communication on

SERIES.

Remarks on the subject were made by Mr. E. B. ELLIOTT.

Mr. W. H. DALL addressed the Society on

THE RESULTS OF RECENT INVESTIGATIONS INTO THE NATURAL HISTORY OF THE CHITONIDÆ,

made independently by Dr. H. von Jhering of Erlangen and himself. He summed up the results of each investigator as follows:—

Dr. Von Jhering finds the sexes of all species examined by him separate; the eggs are impregnated within the body of the parent; in the species he examined the eggs in the ovary are inclosed in a sort of follicle which in *Chiton squemosus* secretes a membrane like a chorion which is provided with radiating thornlike processes. He detects the presence of a dendritic renal organ in the perivisceral cavity, which is lined with a ciliated membrane and opens by a duct in the median line below the anus; the muscular fibres are bunches of fibrillæ invested by a follicular sarcolemma, and those of the pharynx, in his opinion, are not striated in the sense in which those of vertebrates are said to be striated.

Mr. DALL found the sexes always separated in all the species examined (belonging to thirty or forty generic or subgeneric groups), including *Chiton Pallasii*, which Middendorf had supposed to be hermaphrodite. The egg is impregnated in the ovisac, or oviduct, or both, in several species; whether in all or not further material was needed to determine; in none of the species examined was a chorion discovered; nevertheless it may exist in some and not in others. Mr. DALL detected the renal organ in many species, but it was very small, or even perhaps abortive in some species; he did not discover the excretory external opening, but had not sought especially for it. An oviduct (or pair of oviducts) exists demonstrably in some species, having a small plain opening on each side of the tail of the animal; in others no oviduct could be discovered, and the small opening was replaced by a larger fenestrated opening, apparently giving free access to the water into the perivisceral cavity. From these openings the eggs had been seen to be ejected by Dr. Carpenter and Prof. Verrall, and before ejection had developed to such an extent that the embryonic eyes were plainly visible. (See *Bulletin Essex Inst.* 1873.)

Mr. DALL saw nothing of striated muscular fibre among the *Chitonidæ* dissected. In none of them was a chitinous jaw found, such as is universal among limpets. The dentition, while differing in detail in different species, was of similar type in all examined, and probably in all chitons. A laminated crop exists in most species. One peculiarity is notable throughout the group. The tendency is to degradation of cephalic characters. In embryo chitons the eyes are well developed and the cephalic portion largely developed. In adult individuals not only are there no eyes, but the tentacles common to most gasteropods are absent, the nerves which are wont to supply these organs with sensibility are also wanting; there is no jaw, the important centres of circulation, respiration, and reproduction are all posteriorly situated. As between different genera of chitons, those which have the rows of branchial tufts shorter than in other genera have the deficiency in the cephalic end of the row; in *Chitonellus*, perhaps the highest form of *Chitonidæ*, the branchiæ are few, large, and collected in a very short bunched-up row close to and on each side of the anus.

The chitons go back to Silurian times and (excluding the cerriped valves, fish-scales, etc., which have been described by Eu-

ropean palæontologists as valves of chitons), all Paleozoic chitons belong to the group without laminæ of insertion to the valves, which are typified by the existing *Leptochitons* of Carpenter, which are confined to arctic and temperate seas so far as known.

Mr. G. K. GILBERT made a communication entitled

THE WASATCH A GROWING MOUNTAIN.

(ABSTRACT.)

In a general way the structure of the Wasatch range is easily described, for although it is complex in detail there is one feature which prevails through its entire length, and is always the important feature. Everywhere it is bounded on the west by a profound fault, and the rocks constituting it have a general dip to the east. The rocks eastward of the fault plane have been uplifted at several epochs, and have been continuously subjected to erosion, so that their remnant, which forms the mountain, exhibits but a small fraction of the entire uplifted mass. Their revealed section includes, according to Mr. King, ten miles in thickness of conforming strata. The rocks westward of the fault plane are not in sight, being buried at an unknown depth beneath the debris worn from the eastward mass. The maximum displacement or throw of the fault is therefore more than ten miles.

Along the plane of this great fault there have been recent movements, and the bluffs or escarpments to which they have given rise bear such relation to the water-marks produced by Lake Bonneville (probably a feature of the Glacial Epoch), that their date is established as post-glacial, or at least post-Bonneville. The movements were not coincident, but were clearly separated by intervals of time, and the latest one was so recent that the escarpment it produced has not yet been thrown down by frost, but stands a grassless cliff of earth.

The total post-Bonneville erosion has not been sufficient to destroy or even greatly impair the Bonneville terraces. Though chiefly composed of gravel and earth, they have not been degraded more than one or two feet at the utmost, and it is safe to say that the rocky summits of the range itself, exposed to fiercer storms but opposing them with harder material, have been degraded on an average not more than five feet. The total post-Bonneville displacement, examined for a distance of 125 miles, averages about fifty feet. The range has therefore been lifted, with reference to the adjacent valley, an amount ten times as great as the altitude lost by erosion; and this has taken place in the latest epoch of geological time—an epoch continuous with the historic. We may fairly say that at the present time the causes to which the mountain is due not merely continue, but are more potent than the causes which tend to obliterate it. It is a *growing* mountain.

Mr. ANTISELL suggested that instead of a rise of the mountain there was a less degree of subsidence on the one side than on the other.

SPECIAL MEETING. MAY 14, 1878.

Vice-President HILGARD in the Chair.

Forty-five members present.

The meeting had been called by Vice-President HILGARD, who with preliminary remarks announced that it was for the purpose of taking action on the occasion of the death of Professor JOSEPH HENRY, President of the Society.

The Secretary of the meeting read a communication from Chief Justice M. R. WAITE, Chancellor of the Smithsonian Institution, announcing the death of Professor JOSEPH HENRY, the Secretary and Director of the Institution, in this city on Monday, May 13th, at 12.10 P. M., and inviting the Philosophical Society of Washington to attend his funeral on Thursday at half-past four o'clock in the afternoon.

On motion of Mr. W. B. TAYLOR, a committee, consisting of Messrs. W. B. TAYLOR, WELLING, and GILL, was appointed to prepare appropriate resolutions.

Remarks on the character and labors of the deceased were made by Messrs. HILGARD, JOHNSON, TONER, ALVORD, ABBE, MASON, GALLAUDET, and GEORGE TAYLOR.

The Committee reported the following resolutions, which were unanimously adopted :—

Resolved, That in the death of Professor JOSEPH HENRY the Philosophical Society of Washington is called to deplore the loss of its venerable and beloved president, who from its first institution, and subsequently from year to year, has been unanimously chosen to the position he filled among us, in deference not only

to the exalted fame which made him the chief ornament of our association, but in grateful tribute as well to the varied philosophical learning, the calm, even-balanced judgment, and the serene wisdom which so admirably qualified him to be the moderator of opinions in a body composed of zealous and independent workers in nearly every department of scientific research.

Resolved, That while we are called to sit in the shadow of a great bereavement, which naturally casts its deepest gloom on those who, like ourselves, were daily admitted to the privilege of his personal friendship and to the precious opportunities afforded by his sagacious and logical suggestions and wide erudition, as well as by his ready co-operation in every enterprise which had for its object the extension of knowledge or the promotion of human welfare, we at the same time feel that we should be culpably insensible to the surviving radiance of the bright example he has set us if, even here, in the presence of his unfilled grave, we did not testify and record our solemn thanksgiving for the length of days accorded to our revered friend and illustrious exemplar, permitted as he was to extend his useful life beyond the period usually allotted to man, and not only filling that life with abundant labors, which have reflected the highest honor on science, but also adorning it with the moral virtues and Christian graces which made him as lovely for the beauty and simplicity of his nature as he was remarkable for the strength and dignity of his high and noble character.

Resolved, That when we transfer our thoughts from the precincts of this Society, within which he has shed so long and so graciously the mild light of his high and varied intelligence, to that wider arena in which he moved as minister and interpreter of nature, plucking out the heart of her hidden mysteries; as teacher of ingenuous youth, quickening in their minds an ardent love of knowledge; as apostle of science, deeply imbued with reverence for his holy calling; as unselfish worker for the Government, serving it even unto death in so many fields of useful and unrewarded activity; and, above all, when we refer to his long and beneficent career as Director of the great institution to which SMITHSON gave his name, but to which HENRY has given the distinctive direction and specific character which compose the chief element of its glory in the past and constitute the highest pledge of its usefulness in the future, we are filled with

admiration, not only for the variety and depth of his lore, and for the amplitude of the intellectual sympathies which enabled our honored head to take "all knowledge for his province," but also for the rare executive talent which, in the sphere of administration, fitted him successfully to touch the springs of original inquiry at almost every point in the wide domains of modern science.

Resolved, That, as we survey the long and splendid career of the great philosopher who has just fallen at his post of duty on the high places of the land, and to whose finished life the seal of death has now been set, amid the universal regrets of his countrymen, shared by the civilized world wherever science has a votary, we shall best prove our love and veneration for his memory, not by indulging in fruitless repinings, but by borrowing inspiration and incentive from the sublime example left us in the purity of his life, and in the beneficence of the works which still follow him, though he has rested from his labors.

Resolved, That, cherishing for his memory a profound admiration and affection, we proffer to his bereaved family our sincerest sympathy and condolence, and that we will attend his funeral as co-mourners in a body.

It was further

Resolved, That the Secretaries transmit copies of these resolutions to the family of Professor HENRY, and to the Regents of the Smithsonian Institution.

144TH MEETING. MAY 25, 1878.

Vice-President WELLING in the Chair.

Thirty-five members and visitors present.

The election of Mr. WALTER HAYDEN GRAVES and Mr. JOSEPH BADGER MARTIN as members of the Society was announced.

Mr. ALVORD read a paper on

"THE INTERSECTION OF CIRCLES AND THE INTERSECTION OF SPHERES."

Remarks were made by Messrs. BAKER, TAYLOR, and HARKNESS.

Mr. NEWCOMB made preliminary remarks on

THE RECENT TRANSIT OF MERCURY,

describing the optical and physical phenomena as noticed by himself and others. He observed the transit with a three-inch telescope, and saw a decided black drop; also a small white spot on the disk of Mercury. He stated that this transit afforded confirmation of LE Verrier's conclusion respecting the increased motion of Mercury's perihelion.

Mr. HARKNESS made remarks on the physical phenomena, stating that he had never seen the black drop, though others have noticed it.

Mr. HALL spoke of the results of observations at the Washington Observatory, and added that observations had been made at nearly sixty stations; but the first contact was generally lost. Photographs were taken at Cambridge, Washington, Ann Arbor, and by a party of French astronomers at Ogden.

145TH MEETING. JUNE 8, 1878.

Vice-President TAYLOR in the Chair.

Twenty members and visitors present.

Mr. J. W. POWELL made a communication on

THE EVOLUTION OF LANGUAGE.

The subject was discussed by Messrs. WELLING, TAYLOR, WARD, FARQUHAR, and GILL.

Mr. E. B. ELLIOTT presented the following communication on

MUSICAL INTERVALS.

(ABSTRACT.)

In the consideration and discussion of musical intervals from a numerical standpoint, it is not unfrequently more convenient to compare the logarithms of the ratios than the ratios themselves, and the simplest form for the logarithmic series to be employed is the number 2. Octaves, it is well known, progress by integral powers of 2. Intermediate tones progress by *fractional* powers of 2.

In the modern major scale the seven intervals which comprise the octave, or by which, proceeding from a fundamental the octave is reached, consist of but three *different* intervals, known respectively as the major interval, minor interval, and semi-interval—the entire octave comprising three of the major, two of the minor, and two of the semi-intervals, so called.

In the major interval there are eight vibrations of the lower in the time of nine vibrations of the upper of the two tones which limit the interval. In the minor interval there are nine vibrations of the lower to ten of the upper, of the two tones which limit the interval. In the semi-interval, so called, there are fifteen vibrations of the lower to sixteen of the upper of the two tones which limit the interval. Thus making three distinct ratios of progress, to wit, $\frac{9}{8}$, $\frac{10}{9}$, $\frac{16}{15}$. The ratio $\frac{9}{8}$ is equivalent to 2, raised to the power 0.170. The ratio $\frac{10}{9}$ is equal to 2, raised to the power 0.152. The ratio $\frac{16}{15}$ is equal to 2, raised to the power of 0.093.

The advantage of employing the logarithms in place of the ratios themselves in making comparisons is, that the operation of comparison is thereby conducted by additions and subtractions instead of by multiplications and divisions, the former processes being obviously the simpler.

The advantage of employing the number 2 as the *base* of the system of logarithms, is the well-known fact that octaves progress by integral powers of 2, hence it is convenient to have the intermediate notes progress by *fractional* powers of *two*.

It may be observed that the semi-intervals, so called, are in fact *greater* than the half of either the major or the minor intervals. The sum of the logarithms—base 2—of the three major intervals, the two minor intervals, and the two semi-intervals (comprising the octave), is necessarily *unity:*

$$\begin{array}{r} 3 \times .170 = 0.510 \\ 2 \times .152 = 0.304 \\ 2 \times .093 = 0.186 \\ \hline 1.000 \end{array}$$

Mr. A. Schott presented an exposition on

A NEW EYE-PIECE FOR OBSERVING PERSONAL EQUATIONS,

and exhibited the instrument, made by an employé of the U. S. Coast Survey.

146TH MEETING. JUNE 22, 1878.

Vice-President WELLING in the Chair.

Thirty-five members and visitors present.

Mr. E. B. ELLIOTT made a communication on

AN ADJUSTMENT OF THE CARLISLE TABLES OF REVERSIONS AND ANNUITIES,

reducing the irregularities by means of equations such that the first and second orders of differences showed no manifest irregularity.

The subject was discussed by Mr. WOODWARD and others.

Mr. B. ALVORD made a communication on

NEW POINTS RESPECTING THE INTERSECTIONS OF CIRCLES AND THE INTERSECTIONS OF SPHERES,

relating especially to the number of solutions when the question is, "To draw a circle which shall cut each of four given circles at the same angle, said angle being unknown;" also, "To draw a sphere which shall cut each of four given spheres at the same angle, said angle being unknown," and the number of solutions.

Mr. HARKNESS made a communication on

THE VELOCITY OF LIGHT AND DETERMINATION OF THE SOLAR PARALLAX.

147TH MEETING. OCTOBER 12, 1878.

Vice-President TAYLOR in the Chair.

Thirty-six members and visitors present.

The election of Mr. CHARLES VALENTINE RILEY, Entomologist of the Agricultural Department, as a member of the Society, was announced.

Mr. S. NEWCOMB read a communication on

OBSERVATIONS OF THE TOTAL SOLAR ECLIPSE, JULY 29, 1878,

remarking on the favorable circumstances for observations in the mountain regions of Nevada, the extraordinary extension of the corona and its distribution, and the supposed discovery by Prof. WATSON of two inter-mercurial planets. Another total eclipse is necessary to verify this discovery.

Remarks were made by Messrs. FRISBY and ABBE respecting their own observations of this eclipse, and by Messrs. E. B. ELLIOTT, COFFIN, FARQUHAR, and WOODWARD, and by Mr. PAUL, who gave a short description of his method of obtaining on a plate of very finely ground glass in the focus of a telescope, a tracing of the image of the solar corona, showing in its outline quite a close correspondence to the photographs of long exposure obained at the same time.

Mr. ABBE presented a letter from Mr. WESTMINISTER S. ABBEY, of New York, inquiring about

A FISH FOUND ON THE FLORIDA COAST.

Mr. GILL stated that the scale and the description showed it to be the *megalops atlanticus* or tarpa described by Cuvier, *Histoire Naturelle des Poissons*, and by Agassiz in 1857.

Mr. E. B. ELLIOTT made a communication on

MERIDIONAL TIME FOR RAILWAY AND TELEGRAPHIC PURPOSES,

proposing the adoption of meridians differing one hour in longitude.

Remarks were made by Mr. COFFIN, questioning the convenience of the system for railways and for telegraphic purposes. The allowance for difference in time is easily made. For railways the meridian of some prominent place may, and has been, used for extensive sections of country.

Mr. T. N. GILL made a communication on

THE FAMILY OF CERATIIDS.

148TH MEETING. OCTOBER 26, 1878.

Vice-President TAYLOR in the Chair.

Fifty members and visitors present.

James Clark

Mr. J. C. WELLING read the following address on the

LIFE AND CHARACTER OF JOSEPH HENRY,

and Mr. W. B. TAYLOR gave an historical account of

HIS SCIENTIFIC LABORS.

6

LIFE AND CHARACTER OF JOSEPH HENRY.

JOSEPH HENRY was born in Albany, N. Y., on the 17th of December, 1799. His grandparents on both his father's and mother's side emigrated from Scotland, and landed in this country on the 16th of June, 1775, the day before the battle of Bunker's Hill. At the age of seven or earlier, for what reason is unknown, he went to live with his maternal grandmother, who resided at Galway, in the county of Saratoga, N. Y., and his father having died soon afterwards, he continued to dwell for years under her roof. At Galway he attended the district school, of which one Israel Phelps was the master, and having there learned how "to write and cipher, too," he was placed at the early age of ten in a store kept in the village by a Mr. Broderick. Receiving from his employer every token of kindness, and, indeed, of paternal interest in his welfare, the boy-clerk, already remarkable for his handsome visage, his slender figure, his delicate complexion, and his vivacious temper, became a great favorite with his comrades, who, according to the customs of the village store, were wont to saunter about the door in summer, and to gather round the stove in winter for the interchange of such trivial gossip as pertains to village life. Though released at this time for the half of each day from the duty of waiting in the store, that he might attend the sessions of the common school in the afternoon, it does not appear that he had as yet evinced any taste for books, notwithstanding the fact, as he afterwards recalled, that his young brain was even then troubled at times with the "malady of thought," as he lost himself in the mazes of revery or speculation about God and creation—"those obstinate questionings of sense and outward things," which the philosophical poet of England has

described as the natural misgivings of a "creature moving about in worlds not realized." "Delight and liberty," as was natural to a bright boy in the full flush of his animal spirits, still remained the simple creed of his childhood, until one day his pet rabbit escaped from its warren, and ran into an opening in the foundation of the village church. Finding the hole sufficiently large to admit of pushing his person through it, he followed on all fours in eager pursuit of the fugitive, when his eyes were attracted in a certain direction by a glimmer of light, and groping his way towards it, beneath the church, he discovered that it proceeded from a crevice which led into the vestibule of the building, and which opened immediately behind a bookcase that had been placed in the vestibule, as the depository of the village library. Working his way to the front of the bookcase, he found himself in the presence of all the literature stored on its shelves, and on his taking down the first book which struck his eye, it proved to be Brooke's Fool of Quality, a work of fiction in which views of practical life and traits of mystical piety are artfully blended, insomuch that even John Wesley was inclined to except it from the *auto-da-fé*, which, after the manner of the curate and barber in the story of Don Quixote, he would have gladly performed upon the less edifying products of the novel-writing imagination. Poring over the pages of this fascinating volume, young Henry forgot the rabbit in quest of which he had crept beneath the church. It was the first book he had ever read with zest, because it was the first book he had ever read at the impulse of his "own sweet will." Mrs. Browning has told us that we get no good from a book by being ungenerous with it, by calculating profits—"so much help by so much reading."

> ——— "It is rather when
> We gloriously forget ourselves, and plunge
> Soul-forward, head-long, into a book's profound,
> Impassioned for its beauty and salt of truth—
> 'Tis then we get the right good from a book."

Such was the "soul-forward, head-long plunge" which the boyish Henry now first took in the waters of romance, rendered only the sweeter to him, it may be, because, without affront to innocence, they took the flavor of "stolen waters" from the stealth with which they were imbibed. From that time forth he made frequent visits to this library, by the same tortuous and under-

ground passage, reading by preference only works of fiction, the contents of which he retailed to listening comrades around the stove by night, until, in the end, his patron, who shared in his taste for such "light reading," procured for him the right of access to the library in the regular way, and no longer by the narrow fissure in the rear of the bookcase.

At the age of fifteen he left the store of Mr. Broderick in Galway, and, returning to the place of his birth, entered a watchmaker's establishment in Albany, but finding nothing congenial to his taste in the new pursuit, he soon abandoned it. At this time he had formed a strong predilection for the stage. Two or three years before, while living at Galway, he had seen a play for the first time, on the occasion of a casual visit to Albany, and the impression it made upon his mind was as vivid as that left by the perusal of his first novel. He described and re-enacted its scenes for the wonderment of the Galway youth, and now that he was living in Albany, he could give full vent to his new inclination. His spare money was all spent in theatrical amusements, until at length he won his way behind the scenes, and procured admission to the green room, where he learned how to put a play on the boards, and how to produce the illusion of stage effects. In the skill with which he learned thus early to handle the apparatus of the stage, we may discern, perhaps, the first faint prelude of the skill to which he subsequently attained in handling the "levers and screws" with which, according to Goethe, the experimental philosopher seeks to extort from nature the revelation of her mysteries.

Invited at this period of his life to join a private theatrical association in Albany, known by the name of "The Rostrum," the young enthusiast soon distinguished himself among his fellow-members of riper years by the ingenuity of his dramatic combinations, and the felicity of his scenic effects, insomuch that he was made President of the Society. Meanwhile, the watchmaker had left Albany, and young Henry, no longer having the fear of the silversmith's file and crucible before his eyes, was left free to follow the lead of his dramatic tastes and aspirations. He dramatized a tale, and prepared a comedy; both of which were acted by the association. Indeed, so much was he absorbed in this new vocation, that our amateur Roscius seemed, according to all outward appearance, in a fair way of making a place for

himself among the "periwig-pated fellows who tear a passion to tatters" on the stage; or, at the best, of taking rank with the great dramatic artists who, standing in front of the garish footlights, "hold the mirror up to nature," in a sense far different from that of the experimental philosopher, standing in the clear beams of that *lumen siccum* which Bacon has praised as the light that is best of all for the eyes of the mind. But in the midst of these "unintelligible dumb shows," under which the unique and original genius of Henry had thus far seemed to be masquerading, we have now come to the time when his mind underwent a great transfiguration, which revealed its native brightness, and a transfiguration as sudden as it was great.

Minds richly endowed, if started at first on a wrong track, may sometimes have, it would seem, an intellectual conversion as marked as that moral conversion which is often visible in the lives of great saints. It certainly was so in the case of Henry. Overtaken in the sixteenth year of his age by a slight accident, which detained him for a season within doors, he chanced, in search of mental diversion, to cast his eyes upon a book which a Scotch gentleman, boarding with his mother, had left upon the table in his chamber. It was Dr. Gregory's Lectures on Experimental Philosophy, Astronomy, and Chemistry. It commences with an address to the young reader, in which the author stimulates him to deeper inquiry concerning the familiar objects around him. "You throw a stone," he says, "or shoot an arrow upwards into the air; why does it not go forward in the air, and in the direction you give it? What force is it that presses it down to the earth? Why does flame or smoke always mount upward? You look into a clear well of water, and see your own face and figure, as if painted there; why is this? You are told it is done by reflection of light. But what is reflection of light?" etc. etc. These queries certainly are very far from representing the *prudens quæstio* of Bacon in even its most elementary form, but they opened to the mind of young Henry an entirely "new world of thought and enjoyment." His attention was enchained by this book as it had not been enchained by the fiction of Brooke, or by the phantasmagoria of the drama.* The book did for him

* He soon became so much interested in this book that its owner gave it to him, and in token of the epoch it had marked in his life, Prof. Henry ever afterwards preserved it among the choicest memorials of his boy-

what the spirits did for Faust when they opened his eyes to see the sign of the macrocosm, and summoned him "to unveil the powers of nature lying all around him." Not more effectual was the call which came to St. Augustine, when, as he lay beneath the shadow of the fig-tree, weeping in the bitterness of a contrite soul, he seemed to hear a voice that said to him: "*Tolle, lege; tolle, lege,*" and at the sound of which he turned away forever from the Ten Predicaments of Aristotle, and all the books of the rhetoricians, to follow what seemed to him the "lively oracles of God." No sooner had Henry recovered from his sickness, than, obedient to the new vision of life and duty which had dawned upon him, he summoned his comrades of "the Rostrum" to meet him in conference, formally resigned the office of President, and, in a valedictory address, announced to his associates that, subordinating the pleasures of literature to the acquisition of serious knowledge, he had determined henceforth to consecrate his life to arduous and solid studies.

There are doubtless those who, in the retrospect of Prof. Henry's youth, as contrasted with the rich flower and fruitage of his riper years, will please themselves with curious speculations on what "might have been," if his rabbit had never slipped its inclosure, if there had been no crack in the wall behind the bookcase, or if Gregory's Lectures had never fallen in his way at the critical juncture of his life, much as the great mind of Pascal pleased itself with musing how the fate of Europe might have been changed if the Providential grain of sand in Cromwell's tissue had not sent him to a premature grave; or how the whole face of the earth would have been changed if the nose of Cleopatra had been a little shorter than it was, and so had marred the beauty of face which made her, like another Helen, the *teterrima causa*

hood. In the fly-leaf of the book the following memorandum is found, written by Prof. H. in the year 1837: This book, although by no means a profound work, has, under Providence, exerted a remarkable influence on my life. It accidentally fell into my hands when I was about sixteen years old, and was the first book, with the exception of works of fiction, that I ever read with attention. It opened to me a new world of thought and enjoyment, invested things before almost unnoticed with the highest interest, fixed my mind on the study of nature, and caused me to resolve at the time of reading it that I would devote my life to the acquisition of knowledge.—JOSEPH HENRY.

belli for a whole generation. Such fanciful speculations, it seems to me, are well calculated to import into the philosophy of human life, and into the philosophy of human history, a theory of causation which is as superficial as it is false. As honest Horatio says to Hamlet in the play, when the latter proposes to trace the noble dust of Alexander the Great, in imagination, until perchance it may be found stopping a bung-hole, I feel like saying in the presence of such fine-spun speculations, "'Twere to consider too curiously to consider so." The strong intellectual forces which are organic in a great mind, as the strong moral and political forces which are organic in society, do not depend for their evolution, or for their grand cyclical movements, on the casual vicissitudes which ripple the surface of human life and affairs. To argue in this wise is to mistake occasion for cause, and by confounding what is transient and incidental with what is permanent and pervasive, is to make the noblest life, with its destined ends and ways, the mere creature of accident, and is to convert human history, with its great secular developments, into the fortuitous rattle and chance combinations of the kaleidoscope. We may be sure that Henry was too great a man to have lived and died without making his mark on the age in which his lot was cast, whatever should have been the time, place, or circumstance which was to disclose the color and complexion of his destiny. The strong, clear mind, like the crystal, takes its shape and pressure from the play of the constituent forces within it, and is not the sport of casual influences that come from without.

Armed, however, with his new enthusiasm, the nascent philosopher hastened to join a night school in Albany, but soon exhausted the lore of its master. Encountering next a peripatetic teacher of English grammar, he became, under the pedagogue's drill, so versed in the arts of orthography, etymology, syntax, and prosody, that he started out himself on a grammatical tour through the provincial districts of New York, and returning from this first field of his triumphs as a teacher, he entered the Albany Academy (then in charge of Dr. T. Romeyn Beck) as a pupil in its more advanced studies. Meanwhile, in order to "pay his way" in the academy, he sought employment as a teacher in a neighboring district school, this being, as he afterwards was wont to say, the only office he had ever sought in his life; and in this office he succeeded so well that his salary was raised from $8

for the first month to the munificent sum of $15 for the second month of his service! From pupil in the academy and teacher of the district school, he was soon promoted to the rank of assistant in the academy, and henceforward had ample means for the further prosecution of his studies. Leaving the academy, he next accepted the post of private tutor in the family of the Patroon in Albany, Mr. S. Van Rensselaer; and, devoting his leisure hours to the study of the higher mathematics, in conjunction with chemistry, physiology, and anatomy, he at this time purposed to enter the medical profession, and had made some advances in this direction, when he was called, in the year 1826, to embark in a surveying expedition, set on foot under the auspices of the State government of New York, for the purpose of laying out a road through the southern tier of counties in that State. Starting with his men at West Point, and going through the woods to Lake Erie, he acquitted himself so well in this expedition that his friends endeavored to procure for him a permanent appointment as captain of an engineering corps, which it was proposed to create for the prosecution of other internal improvement schemes, but the bill projected for this purpose having fallen through, Mr. Henry again accepted, though with some reluctance, a vacant chair which was offered him in the Albany Academy.

In connection with the duties of this chair, he now commenced a series of original experiments in natural philosophy—the first connected series which had been prosecuted in this country. Dr. Hare, indeed, had already invented the compound blowpipe, as Franklin before him, by his brilliant but desultory labors, had given an immense impulse to the science of electricity; yet none the less is it true that regular and systematic investigations, designed to push forward the boundaries of knowledge, abreast with the scientific workers of Europe, had hardly been attempted at that era in the United States.

The achievements of Henry in this direction soon began to win for him an increase of reputation as well as an increase of knowledge; but in the midst of the fervors which had come to quicken his genius, he was visited by the fancy (or was it a fact?) that a few of the friends who had hitherto supported him in his high ambition were now beginning to look a little less warmly on his aspirations. Suffering from this source the mental

depression which was natural to a sensitive spirit, no less remarkable for its modesty than for its merit, he found solace in the friendly words of good cheer and hopefulness addressed to him by Mr. William Dunlap.* While one day making, with Mr. Henry, a trip down the Hudson River, on board the same steamboat, Mr. Dunlap observed in the young teacher's face the marks of sadness, and, on learning its cause, he laid his hand affectionately on Henry's shoulder, and closed some reassuring advice with the prophetic words, "Albany will one day be proud of her son." The presage was destined to be abundantly confirmed. Soon afterwards came the call to Princeton College, and, because of the wider career it opened to him, the call was as grateful to Henry as its acceptance was gratifying to the friends of that institution. And shortly before this promotion, a new happiness had come to crown his life in his marriage to the excellent lady who still survives him.

He entered upon the duties of his new post in the month of November, 1832, and bringing with him a budding reputation, which soon blossomed into the highest scientific fame, he became the pride and ornament of the Princeton Faculty. The prestige of his magnets attracted students from all parts of the country; but the magnetism of the man was better far than any work of his cunning hand or fertile brain. It was in Princeton, as he was afterwards wont to say, that he spent the happiest days of his life, and they were also among the most fruitful in scientific discovery. Leaving the record of his particular achievements at this epoch to be told by Mr. Taylor, who is so well qualified to do them justice, I beg leave only to refer to this period in the career of Prof. Henry as that in which it was my good fortune to come, for the first time, under the personal influence of the great philosophical scholar, who, after being my teacher in science during the days of my college novitiate at Princeton, continued during the whole of his subsequent life to honor me with a friendship which was as much my support in every emergency that called for counsel and guidance, as it was at all times my joy and the crown of my rejoicing.

* This Mr. Dunlap had been the manager of the Park Theatre in New York, and combined with his dramatic vocation the pursuits of literature and the painter's art. He wrote the "History of Arts and Designs in the United States," a work which was esteemed a standard one at the date of its first publication in 1834.

In the year 1847, when Prof. Henry was in the forty-eighth year of his age, he was unanimously elected by the Regents of the Smithsonian Institution as its Secretary, or Director. At that time the institution existed only in name, under the organic act passed by Congress for its incorporation, in order to give effect to the bequest of James Smithson, Esquire, of London, who by his last will and testament had given the whole of his property to the United States, to found at Washington, under the name of the "Smithsonian Institution," an establishment for "the increase and diffusion of knowledge among men." It does not need to be said that Prof. Henry did not seek this appointment. It came to him unsolicited, but it came to him from the Board of Regents, not only by the free choice of its members, but also at the suggestion and with the approval of European men of science, like Sir David Brewster, Faraday, and Arago, as also of American scientific men, like Bache and Silliman and Hare. I well remember to have heard the late Geo. M. Dallas (a member of the constituent Board of Regents, by virtue of his office as Vice-President of the United States), make the remark on a public occasion, immediately after the election of Prof. Henry as Director of the Smithsonian Institution, that the Board had not had the slightest hesitation in tendering the appointment to him "as being peerless among the recognized heads of American science."

At the invitation of the Regents he drew up an outline plan of the Institution, and the plan was adopted by them on the 13th of December, 1847. The members of this Society, living, as they do, beneath the shadow of the great institution to which Smithson worthily gave his name and his estate, but of which Henry was at once the organizing brain and the directing hand from the date of its inception down to the day of his death, do not need that I should sketch for them the theory on which it was projected by its first Secretary, or that I should rehearse in detail the long chronicle of the useful and multiform services which in pursuit of that theory it has rendered to the cause of science and of human progress. And, moreover, in doing so I should here again imprudently trench on the province assigned to my learned colleague. But I may be allowed to portray the method and spirit which he brought to the duties of this exacting post, at least so far as to say that he proved himself as great in administration as he was great in original research; as skilful in

directing the scientific labors of others as he was skilful in the conduct of his own. Seizing, as with an intuitive eye, the peculiar genius of an institution which was appointed to "*increase* knowledge" and to "*diffuse*" it "among men," he touched the springs of scientific inquiry at a thousand points in the wide domain of modern thought, and made the results of that inquiry accessible to all, with a catholicity as broad as the civilized world. And the publications of the Smithsonian Institution, valuable as they are, and replete as they are with contributions to human knowledge, represent the least part of his manifold labors in connection with the Institution. His correspondence was immense, covering the whole field of existing knowledge, and ranging, in the persons addressed, from the genuine scientific scholar in all parts of the world, to the last putative discoverer of perpetual motion, or the last embryo mathematician who supposed himself to have squared the circle.

In accepting a post where he was called by virtue of his office to fecundate the labors of other men rather than his own, Prof. Henry distinctly saw that he was renouncing for himself the paths of scientific glory on which he had entered so auspiciously at Albany and Princeton. He once said to me, in one of the self-revealing moods in which he sometimes unbosomed himself to his intimate friends, that in accepting the office of Smithsonian Secretary he was conscious that he had "sacrificed future fame to present reputation." He was in the habit of recalling that Newton had made no discoveries after he was appointed Warden of the Mint in 1695,* and I believe that the remark is historically accurate, unless we should incline with Biot, against the better opinion of Sir David Brewster, to place after that date the "discoveries" which Newton supposed himself to have made in the Scriptural chronology and in the interpretation of the Apocalypse—discoveries which, whenever made, provoked the theological scoff, as they perhaps deserved the theological criticism of

* The effect of the Wardenship on Newton's scientific labors may be seen in the warmth with which he rebuked Flamsteed for purposing to publish, in 1698, the fact that Newton was then engaged on a revision of the Horroxian theory of the moon. Newton wrote: "I do not love to be printed on every occasion, much less to be dunned and teased by foreigners about mathematical things, or to be thought by our own people *to be trifling away my time when I should be about the King's business.*"

the polemical Bishop Warburton. Yet, having convinced himself that it was a duty which he owed to the cause of science, to sink his own personality in the impersonal institution he was called to conduct, Henry never paused for an instant to confer with flesh and blood, but moved "right onward" in the path of duty, with only the more of steadfastness, because he felt that it was for him a path of sacrifice.

How sedulously he strove to maintain the Institution in the high vocation to which he believed it was appointed no less by a sacred regard for the will of its founder than by an intelligent zeal for the promotion of human welfare, is known to you all. And the success with which he resisted all schemes for the impoverishment of the exalted function it was fitted to perform in the service of abstract science, is a tribute at once to his rare executive skill, and to the native force of character which made him a tower of strength against the clamors of popular ignorance and the assaults of charlatanism. Whatever might be the consequences to himself personally, he was determined to magnify *its* vocation and make *it* honorable. And hence I do not permit myself to doubt that during the long period of his administration, as Secretary of the Smithsonian Institution, covering a period of thirty years, he has impressed upon its conduct a definite direction which his successors will be proud to maintain, not simply in reverence for the memory of their illustrious predecessor, but also in grateful recognition of the fruitful works which, in the pursuit of his enlightened plans, will continue to follow him now that he has rested from his labors.

The rest into which he has entered came to him in a green old age, after a life as full of years as it was full of honors. He was not only blest with an old age which was

——— serene and bright,
And lovely as a Lapland night,

but he also had that which, according to the great dramatist, should accompany old age—"As honor, love, obedience, troops of friends." And the manner of his death was in perfect keeping with the manner of his life. Assured for months before the inevitable hour came that his days on earth were numbered, he made no change in his daily official employments, no change in his social and literary diversions. None was needed. Surprise, I learn, has been expressed that in the full prospect of death he

should have "talked" so little about it. But the surprise is quite unfounded. Prof. Henry was little in the habit of talking about himself at any time. Yet to his intimate friends he spoke freely and calmly about his approaching end. Two weeks before he died he said to one such, a gentleman from New York, to whom he was strongly attached: "I may die at any moment. I would like to live long enough to complete some things I have undertaken, but I am content to go. I have had a happy life, and I hope I have been able to do some good." In an hour's conversation which I had with him, six days before he died, he referred to the imminence of his death with the same philosophic and Christian composure. And perfectly aware as he was, on the day before he died, and on the day of his death, that he had already entered the Dark Valley, he feared no evil as he looked across it, but, poised in a sweet serenity, preserved his soul in patience, at an equal remove from rapture on the one hand, or anything like dismay on the other. For his friends he had even then the same benignant smile, the same warm pressure of the hand, and the same affable converse as of yore. With the astronomer, Newcomb, he pleasantly and intelligently discoursed about the then recent transit of Mercury—not unheedful of the great transit he was making, but giving heed none the less to every opportunity for the inquiry of truth. Towards the attendants watching around his couch he was as observant as ever of all the "small sweet courtesies" which marked consideration for others rather than for himself even in the supreme moment of his dissolution. The disciples of Socrates recalled with a sort of pathetic wonder at the calm and intrepid spirit of their dying master, that as the chill of the fatal hemlock was stealing towards his heart, he uncovered his face to ask that Crito should acquit him of a small debt he owed to Esculapius; and so in like manner I recall that our beloved chief did not forget in the hour of his last agony to make provision for the due despatch of a letter of courtesy, which on the day before he had promised to a British stranger.

And so in the full possession of all his great mental powers—in his waking hours filled with high thoughts and with a peace which passed all understanding; in his sleep stealing away

"To dreamful wastes where footless fancies dwell,"

and talking even there of experiments in sound on board the

steamer Mistletoe, or haply taking note of electric charges sent through imaginary wires at his bidding*—the soul of Joseph Henry passed away from the earth which he had blessed and brightened by his presence. He died ten minutes after twelve o'clock, on the 13th of May, 1878.

From these imperfect notes on the Life of Prof. Henry, I pass to consider some of his traits and characteristics as a man.

He was endowed with a physical organization in which the elements that composed it were not only fine and finely mixed, but were cast in a mould remarkable for its symmetry and manly beauty. The perfection of his "outward man" was not unworthy of the "inward man" whom it enshrined, and if, as a church father has phrased it, "the human soul is the true Shechinah," it may none the less be said that these "fleshly nooks" of ours never appear to so much advantage as when, transfigured by this Shechinah, they offer to the informing spirit a temple which is as stately as it is pure. When Dr. Bentley was called to write the epitaph of Cotes (that brilliant scholar of whom Newton said that, "if he had lived we might have known something"), the accomplished master of words thought it not unmeet to record that the fallen Professor, who had been snatched away by a premature death, was only "the more attractive and lovely because the virtues and graces which he joined to the highest repute for learning were embellished by a handsome person." The same tribute of admiration might be paid with equal justice to the revered Professor whose "good gray head" has just vanished from our sight.

The fascination of Prof. Henry's manner was felt by all who came within the range of its influence—by men with whom he daily consorted in business, in college halls, and in the scientific academy; by brilliant women of society who, in his gracious presence, owned the spell of a masculine mind which none the less was feminine in the delicacy of its perceptions and the purity of

* Prof. Henry took great delight in the acoustical researches which, during the closing years of his life, he made at sea, on board the steamer Mistletoe, while it was in electricity that he won his first triumphs as a scientific man. That his first love and last passion in science still filled his thoughts in his dying moments was attested by the words which even then fell from his lips, in sleep.

its sensibilities; by children, who saw in the simplicity of his unspoiled nature a geniality and a kindliness which were akin to their own. A French thinker has said that *à mesure qu'on a plus d'esprit on trouve qu'il y a plus d'hommes originaux.* It was the breadth and catholicity of Henry's intelligence which enabled him to find something unique and characteristic in persons who were flat, stale, and unprofitable to the average mind.

Gifted with a mental constitution which was "feelingly alive to each fine impulse," he possessed a high degree of æsthetic sensibility to the beautiful in nature and in art. It cannot be doubted that a too exclusive addiction to the analytic and microscopic study of nature, at the instance of science, has a tendency to blunt in some minds a delicate perception for the "large livingness" of Nature, considered as a source of poetic and moral inspiration, but no such tendency could be discovered in the intellectual habitudes of Prof. Henry. To a mind long nurtured by arts of close and critical inquiry into the logic of natural law he none the less united a heart which was ever ready to leap with joy at "the wonder and bloom of the world." When on the occasion of his first visit to England, in the year 1837, he was travelling by night in a stage-coach through Salisbury Plain, he hired the driver to stop, while all his fellow-passengers were asleep, that he might have the privilege of inspecting the ruins of Stonehenge, as seen by moonlight, and brought away a weird "sense of mystery" which followed him in all his after life. At a later day, in the year 1870, after visiting the Aar Glacier, the scene of Prof. Agassiz's well-known labors, he crossed over the mountain to the Rhone Valley, until, at a sudden turn of the road, he came full in the presence of the majestic Glacier of the Rhone. For minutes he stood silent and motionless; then, turning to the daughter who stood by his side, he exclaimed, with the tears running down his cheeks: "This is a place to die in. We should go no further."

And as he rejoiced in natural scenery so also was he charmed with the beauties of art, whether as seen in "the well-stained canvas or the featured stone," and hence it was that he felt as much at home in the *atelier* of the painter or sculptor as in the laboratory of the chemist or the apparatus room of the natural philosopher, and exulted as sincerely in the Louvre or the Corcoran Gallery of Art as in the cabinet of the mineralogist or the museum of the naturalist.

He was as remarkable for the simplicity of his nature as for the breadth of his mind and the acumen of his intellect. Those who analyze the nature and charm of simplicity in a great mind suppose themselves to find the secret of both in the fact that simplicity, allied with greatness, works its marvels with a sweet unconsciousness of its own superior excellence, and it works them with this unconsciousness because it is greater than it knows. Talent does what it can. Genius does what it must. And in this respect, as an English writer has said, there is a great analogy between the highest goodness and the highest genius; for under the influence of either, the spirit of man, "whenever it lifts up its head and shakes its locks," may scatter light and splendor around it, without admiring itself or seeking the admiration of others. And it was in this sense that the simplicity of Henry's nature expressed itself in acts of goodness and in acts of high intelligence, with a spontaneity which hid from himself the transcendent virtue and dignity of the work he was doing; and hence all his work was done without the slightest taint of vanity or tarnish of self-complacency.

As might be expected, he was a fervent lover of the best literature. His acquaintance with the English poets was not only wide but intimate. His memory was stored with choice passages, didactic, sentimental, witty, and humorous, which he reproduced at will on occasions when they were apt to his purpose. His familiarity with fiction dated, as we have seen, from early boyhood, and in this fountain of the imagination he continued to find refreshment for the "wear and tear" of the hard and continuous thought to which he was addicted in the philosopher's study. His knowledge of history was accurate, and it was not simply a knowledge of facts, but a knowledge of facts as seen in the logical coherence and rational explanation which make them the basis of historic generalization. The genesis of the Greek civilization was a perpetual object of interest to his speculative mind, as called to deal with the phenomena of Grecian literature, art, philosophy, and polity.

He was a terse and forcible writer. If, as some have said, it is the perfection of style to be colorless, the style of Henry might be likened to the purest amber, which, invisible itself, holds in clear relief every object it envelops. Without having that fluent delivery which, according to the well-known comparison

of Dean Swift, is rarely characteristic of the fullest minds, he was none the less a pleasing and effective speaker—the more effective because his words never outran his thought. We loved to think and speak of him as "the Nestor of American Science," and if his speech, like Nestor's, "flowed sweeter than honey," it was due to the excellent quality of the matter rather than to any rhetorical facility of manner.

He was blest with a happy temperament. He recorded in his diary, as a matter of thanksgiving, that through the kindness of Providence he was able to forget what had been painful in his past experiences, and to remember only and enjoy that which had been pleasurable. The same sentiment is expressed in one of his letters. Armed with this sunny temper which, like the dial, "marks only the hours that shine," he was in his family circle a perpetual benediction. And, in turn, he was greatly dependent on his family for the sympathy and watch-care due in a thousand small things to one who never "lost the child-like in the larger mind." His domestic affections were not dwarfed by the exacting nature of his official duties, his public cares, or his scientific vigils. He had none of that solitary grandeur affected by isolated spirits who cannot descend to the tears and smiles of this common world. He was never so happy as when in his home he was communing with wife and children around the family altar. He made them the confidants of all his plans. He rehearsed to them his scientific experiments. He reported to them the record of each day's adventures. He read with them his favorite authors.* He entered with a gleeful spirit

* The following extract from a diary, kept by one of his daughters, is descriptive of his habits under this head: "Had father with us all the evening. I modelled his profile in clay while he read Thomson's Seasons to us. In the earlier part of the evening he seemed restless and depressed, but the influence of the poet drove away the cloud, and then an expression of almost childlike sweetness rested on his lips, singularly in contrast yet beautifully in harmony with the intellect of the brow above."

Or take this extract from the same diary: "We were all up until a late hour, reading poetry with father and mother, father being the reader. He attempted 'Cowper's Grave,' by Mrs. Browning, but was too tender-hearted to finish the reading of it. We then laughed over the Address to the Mummy, soared to heaven with Shelley's Skylark, roamed the forest with Bryant, culled flowers from other poetical fields, and ended with Tam O'Shanter. I took for my task to recite a part of the latter from memory, while father corrected, as if he were 'playing schoolmaster.'"

into all their joys; with a sympathetic heart into all their sorrows. And while thus faithful to the charities of home, he was intensely loyal to his friends, and found in their society the very cordial of life. Gracious to all, he grappled some of them to his heart with hooks of steel. The friendship, fed by a kindred love of elegant letters, which still lends its mellow lustre to the names of Cicero and Atticus, was not more beautiful than the friendship, fed by kindred talents, kindred virtues, and kindred pursuits, which so long united the late Dr. Bache and Professor Henry in the bonds of a sacred brotherhood. And this was but one of the many similar intimacies which came to embellish his long and useful career.

His sense of honor was delicate in the extreme. It was not only that "chastity of honor which feels a stain like a wound," but at the very suggestion of a stain it recoiled as instantly as the index finger of Mr. Edison's tasimeter at the "suspicion" of heat. I met him in 1847, when, soon after his election as Secretary of the Smithsonian Institution, he had just been chosen to succeed Dr. Hare as Professor of Chemistry in the Medical Department of the University of Pennsylvania, at a salary double that which he was to receive in Washington, and with half the year open to free scientific investigation, because free from professorial duties. It was, he said, the post which, of all others, he could have desiderated at that epoch in his scientific life, but his honor, he added, forbade him to entertain, for a moment, the proposition of accepting it after the obligations under which he had come to the interests represented by the Smithsonian Institution. At a later day, after he had entered on his duties in Washington, and found the position environed with many difficulties, Mr. Calhoun came to him, and urged his acceptance of a lucrative chair in a Southern College, using as a ground of appeal the infelicities of his present post, and the prospect of failing at last to realize the high designs he had projected for the management of the Smithsonian Institution. Admitting that it might be greatly to his comfort and advantage at that time to give up the Smithsonian, he declined at once to consider the proposal made to him, on the ground that his "honor was committed to the Institution." Whereupon Mr. Calhoun seized his hand, and exclaimed, "Professor Henry, you are a man after my own heart."

When in 1853, and again in 1867, he was entreated to accept the Presidency of Princeton College, the College of his love, and the scene of his "happiest days," he instantly turned away from the lure, as feeling that he could not love the dear old College so much if he loved not more the honor and duty which bound him to the establishment in Washington, with which, for good or for evil, he had wedded his name and fortune. And in all other concerns, from the greatest to the least, he seemed like one

> "Intent each lurking frailty to disclaim,
> And guard the way of life from all offence,
> Suffered or done."

The "Man of Ross," portrayed by the pencil of Pope, was not more benevolent in heart or act than Professor Henry. His bounty was large and free. The full soul mantled in his eyes at every tale of woe, and the generous hand was quick to obey the charitable impulses of his sympathetic nature. This benevolent spirit ran like a silver cord through the tissue of his life, because it was interwoven in the very warp and woof of his being, and because it was kept in constant exercise. It appeared not only in acts of kindness to the poor and afflicted, but interpenetrated his whole demeanor, and informed all his conduct wherever he could be helpful to a fellow-man. He did good to all as he had opportunity, from "the forlorn and shipwrecked brother," who had already failed in the voyage of life, to the adventurous young mariner who sought his counsel and guidance for the successful launching of his ship from its ways. Many are the young men, who, in all parts of the land, could rise up to-day and call him blessed, for the blessing he brought to them by the kind word spoken and the kind deed done, each in its season.

Unselfishness was a fundamental trait in the character of Professor Henry, and he made the same trait a fundamental one in his conception of the philosopher's high calling. The work of scientific inquiry was with him a labor of love, not simply because he loved the labor, but because he hoped by it to advance the cause of truth and promote the welfare of man. He never dreamed of profiting by any discovery he made. He would not even have his salary increased, so tenaciously did he hold to the Christ-like privilege of living among men "as one that serveth." This was a crown which he would let no man take from him. To the

Government he freely gave, in many spheres of public usefulness, all the time he could spare from his official duties. And it was in one of these subsidiary public labors, as Chairman of the Light-House Board, that he contracted, as he believed, the disease which carried him to the grave.

A sense of rectitude presided over all his thoughts and acts. He had so trained his mind to right thinking, and his will to right feeling and right doing, that this absolute rectitude became a part of his intellectual as well as moral nature. Hence in his methods of philosophizing he was incapable of sophistical reasoning. He sat at the feet of nature with as much of candor as of humility, never importing into his observations "the human being's pride," or the "mystical predominance" of an overweening fancy. He was sober in his judgments. He made no hasty generalizations. His mind seemed to turn on "the poles of truth."

I could not dwell with enough of emphasis on this crowning grace of our beloved friend if I should seek to do full justice to my conception of the completeness it gave to his beautiful character. But happily for me I need dwell upon it with only the less of emphasis because it was the quality which, to use a French idiom, "leaped into the eyes" of all who marked his walk and conversation. In the crystal depths of a nature like his, transparent in all directions, we discern as well the felicity as the beauty of that habit of mind which is begotten by the supreme love of Truth for her own sake—a habit which is as much the condition of intellectual earnestness, thoroughness, and veracity in penetrating to the reality of things, as of moral honesty, frankness, sincerity, and truthfulness in dealing with our fellow-men. The great expounder of the Nicomachean Ethics has taught us, and one of our own moralists has amplified the golden thesis,* that high moral virtue implies the habit of "just election" between right and wrong, and that to attain this habit we need at once an intelligence which is impassioned and an appetite which is reflective. And so in like manner all high intellectual virtue implies a habit of just election between truth and error—an election which men make, other things being equal, according to the degree in which their minds are enamored with the beauty of truth, as also in proportion to the degree in which their appetencies for knowl-

* Dr. James H. Thornwell: Discourses on Truth.

edge have been trained to be reflective and cautious against the enticements of error. I never knew a man who strove more earnestly than Henry to make this just election between right and wrong, between truth and error, or who was better equipped with a native faculty for making the wise choice between them. He had brought his whole nature under the dominion of truthfulness.

But while thus eager and honest in the pursuit of truth he had nothing controversial in his temper. It was a favorite doctrine of his that error of opinion could be most successfully combated, not by the negative processes of direct attack, rousing the pride and provoking the contumacy of its adherents, but rather by the affirmative process of teaching, in meekness and love, the truth that is naturally antagonistic to it. The King of Sweden and Norway made him a Knight of St. Olaf, but St. Olaf's thunderous way of propagating Christianity—by battering down the idols of Norway with Thor's own hammer—is not the way that his American votary would have selected. There was nothing iconoclastic in Henry's zeal for truth. He believed that there is in all truth a self-evidencing quality, and a redemptive power which makes it at once a potent and a remedial force in the world. Hence he never descended to any of those controversies which, in the annals of science, have sometimes made the *odium scientificum* a species of hatred quite as distinct, and quite as lively, too, as its more ancient congener, the *odium theologicum*. When once it was sought to *force* a controversy of this kind upon him, and when accusations were made which seemed to affect his personal honor, as well as the genuineness of his scientific claims, he referred the matter for adjudication to the Regents of the Smithsonian. Their investigation and their report dispensed him from the necessity of self-defence. The simple truth was his sufficient buckler. And this equanimity was not simply the result of temperament. It sprang from the largeness of his mind, as well as from the serious view he took of life and duty. He was able to moderate his own opinions, because, in the amplitude of his intellectual powers, he was able to be a moderator of opinions in the scientific world. You all know with what felicity and intellectual sympathy he presided over the deliberations of this Society, composed as it is of independent scientific workers in almost every department of modern research. Alike in the judicial temper of

his mind and in the wide range of his acquisitions he was fitted to be, as Dante has said of Aristotle, "the master of those who know."

And this power of his mind to assimilate knowledge of various kinds naturally leads me to speak of his skill in imparting it. He was a most successful educator. He had many other titles of honor or office, but the title of *Professor* seemed to rank them all, for everybody felt that he moved among men like one anointed with the spirit and power of a great teacher. And he had philosophical views of education, extending from its primary forms to its highest culminations—from the discipline of the "doing faculties" in childhood, to the discipline of the "thinking faculties" in youth and manhood. No student of his left the Albany Academy, in the earlier period of his connection with that institution, without being thoroughly drilled in the useful art of handling figures, for then and there he taught the rudimental forms of arithmetic, not so much by theory as by practice. No student of his left Princeton College without being thoroughly drilled in the art of thinking, as applied to scientific problems, for then and there he was called to indoctrinate his pupils in the rationale as well as in the results of the inductive method. And I will venture to add that no intelligent student of his at Princeton ever failed, in after life, to recognize the useful place which hypothesis holds in labors directed to the extension of science, or failed to discriminate between a working hypothesis and a perfected theory.

Pausing for a moment at this stage in the analysis of Professor Henry's mental and moral traits, I cannot omit to portray the effect produced on the observer by the happy combination under which these traits were so grouped and confederated in his person as to be mutual complements of each other. Far more significant than any single quality of his mind, remarkable as some of his qualities were, was the admirable equipoise which kept the forces of his nature from all interference with the normal development of an integral manhood. He was courtly in his manners, but it was the courtliness which springs from "high thoughts seated in a heart of courtesy," and had no trace of affectation or artificiality; he was fastidious in his literary and artistic tastes, but he had none of that dilettantism which is "fine by defect and delicately weak;" he was imbued with a simplicity of heart

which left him absolutely without guile, yet he was shrewd to protect himself against the arts of the designing; he was severe in his sense of honor without being censorious; he was benevolent yet inflexibly just; quick in perception yet calm in judgment and patient of labor; tenacious of right without being controversial; benignant in his moral opinions yet never selling the truth; endowed with a strong imagination yet evermore making it the handmaid of his reason; a prince among men yet without the slightest alloy of arrogance in the fine gold of his imperial intellect; in a word, good in all his greatness, he was, at the same time, great in all his goodness. Such are the limitations of human excellence in most of its mortal exhibitions that transcendent powers of mind, or magnificent displays of virtue exerted in a single direction, are often found to owe their "splendid enormity" to what Isaac Taylor has called "the spoliation of some spurned and forgotten qualities," which are sacrificed in the pursuit of a predominant taste, or an overmastering ambition.* The "infirmities of genius" often attest in their subjects the presence of a mental or moral atrophy, which has hindered the full-orbed development of one or more among their mental and moral powers. But in Professor Henry no one quality of mind or heart seemed to be in excess or deficiency as compared with the rest. All were fused together into a compactness of structure and homogeneity of parts which gave to each the strength and grace imparted by an organic union. And hence, while he was great as a philosopher he was greater as a man, for, laying as he did all the services of his scientific life on the altar of a pure, complete, and dignified manhood, we must hold that the altar which sanctified his gifts was greater than even the costliest offerings he laid upon it.

It will not be expected that I should close this paper without referring to the religious life and opinions of Prof. Henry. If in moral height and beauty he stood like the palm tree, tall, erect, and symmetrical, it is because a deep religious faith was the tap-root of his character. He was, on what he conceived to be

* The phrase, as originally applied by Taylor, is descriptive of certain incomplete ethical systems, but it is equally applicable to certain typical exemplifications of human character, in which "the strength and the materials of six parts of morality have been brought together wherewith to construct a seventh part."

rational grounds, a thorough believer in theism. I do not think he would have said, with Bacon, that he "had rather believe all the fables in the Legend, the Talmud, and the Alcoran, than that this universal frame is without a mind," for he would have held that in questions of this kind we should ask not what we would "rather believe," but what seems to be true on the best evidence before us. He was in the habit of saying that, next to the belief in his own existence, was his belief in the existence of other minds like his own, and from these fixed, indisputable points, he reasoned, by analogy, to the conclusion that there is an Almighty Mind pervading the universe. But when from the likeness between this Infinite Mind and the finite minds made in His image, it was sought, by *a priori* logic, or by any preconceived notions of man, to *infer* the methods of the Divine working, or the final causes of things, he suspected at once the intrusive presence of a false, as well as presumptuous, philosophism, and declined to yield his mind an easy prey to its blandishments. To his eyes much of the free and easy teleology, with which an under-wise and not over-reverent sciolism is wont to interpret the Divine counsels and judgments, seemed little better than a Brocken phantom—the grotesque and distorted image of its own authors, projected on mist and cloud, and hence very far from being the inscrutable teleology of Him whose glory it is to conceal a thing, and whose ways are often past finding out, because His understanding is infinite.

As Prof. Henry was a believer in theism, so also was he a believer in Revealed Religion—in Christianity. He had not made a study of systematic, or of dogmatic, theology, as they are taught in the schools, and still less was the interest he took in polemical divinity, but he did have a theology which, for practical life, is worth them all—the theology of a profound religious experience. He was a fresh illustration of Neander's favorite saying: *Pectus facit theologum.* The adaptation of the Christian scheme to the moral wants of the human soul was the palmary proof on which he rested his faith in the superhuman origin of that scheme The plan had to him the force of a theory which is scientific in its exact conformity to the moral facts it explains, when these facts are properly known and fully understood.

Hence he was little troubled with the modern conflict between science and religion. History, as well as reason and faith, was

here his teacher. He saw that the Christian church had already passed through many epochs of transition, and that the friction incident to such transition periods had only brushed away the incrustations of theological error, and heightened the brightness of theological truth. In a world where the different branches and departments of human knowledge are not pushed forward *pari passu*—where "knowledge comes but wisdom lingers"—he held it nothing strange that the scientific man should sometimes be unintelligible to the theologian, and the theologian unintelligible to the scientific man. He believed, with the old Puritan, that "the Lord has more truth yet to break out of His holy word" than the systematic theologian is always ready to admit; and as the humble minister and interpreter of nature, he was certain that the scientific man has much truth to learn of which he is not yet aware. There must needs be fermentation in new thought, as in new wine, but the vintage of the brain, like the vintage of the grape, is only the better for a process which brings impurities to the surface, where they may be scummed off, and settles the lees at the bottom, where they ought to be. It is under the figure of a vintage that Bacon describes the crowning result of a successful inductive process. When this process has been completed in any direction, it remains for a wider critical and reconciling philosophy to bring the other departments of knowledge into logical relation and correspondence with the new outlook that has been gained on nature and its phenomena.

Erasmus tells us in his Praise of Folly, mingling satire with the truth of his criticism, that in order to understand the scholastic theology of his day, it was necessary to spend six-and-thirty years in the study of Aristotle's physics, and of the doctrines of the Scotists. What a purification of method has been wrought in theology since the times of Erasmus! And for that purification the Church is largely indebted to the methodology of modern science, in clearing up the thoughts, and rationalizing the intellectual processes of men. The gain for sound theology is here unspeakable, and amply repays her for the heavy baggage she has dropped by the way at the challenge of science—baggage which only impeded her march, without reinforcing her artillery.

Hence, as a Christian philosopher, Prof. Henry never found it necessary to lower the scientific flag in order to conciliate an obscurantist theology, and he never lowered the Christian flag in

order to conciliate those who would erect the scientific standard over more territory than they have conquered. He had none of that spirit which would rather be wrong with Plato than right with anybody else. He wanted to follow wherever truth was in the van. But better than most men, I think, he knew how to discriminate between what a British scholar calls the duty of "following truth wherever it leads us, and the duty of yielding to the immediate pressure of an argument." He saw, as the same writer adds, that for whole generations "the victory of argument may sway backwards and forwards, like the fortune of single battles," but the victory of truth brings in peace, and a peace which comes to stay. He swept the scene of conflict with the field-glass of a commander-in-chief, and did not set up his trophies because of a brilliant skirmish on the picket lines of science. But he believed in the picket line, and rejoiced in every sharpshooter who fought with loyalty to truth in the forefront of the scientific army.

A man of faith, Prof. Henry was a man of prayer. But his views of prayer were perhaps peculiar in their spirituality. There was nothing mechanical or formal in his theory of this religious exercise. He held that it was the duty and privilege of enlightened Christians to live in perpetual communion with the Almighty Spirit, and in this sense to pray without ceasing. Work was worship, if conducted in this temper. He accepted all the appointments of nature and Providence as the expressions of Infinite Wisdom, and so in everything gave thanks.* He believed that

* The "sweet reasonableness" into which he had schooled his temper was manifested by the great trial which befell him in the year 1865, when the Smithsonian building suffered from the ravages of a fire, which destroyed all the letters written down to that date by Prof. Henry, as Smithsonian Secretary, in reply to innumerable questions relating to almost every department of knowledge. Besides, the Annual Report of the Institution in manuscript, nearly ready for the press, a valuable collection of papers on meteorology, with written memoranda of his own to aid in their digest, and countless minutes of scientific researches which he purposed to make, all perished in the flames. Yet he was more concerned about the loss of Bishop Johns's library, which had been entrusted to his care, than about the loss of his own papers and records. Referring to the latter in a letter written to his friend, Dr. Torrey, a few days after the fire, he held the following language: "A few years ago such a calamity would have paralyzed me for future efforts, but in my present view of life I take it as the dispensation of a kind and wise Providence, and trust that it will work to my spiritual advantage."

familiarity with the order of nature and scientific assurance of its uniformity need not and should not tend to extinguish the instinct, or abolish the motives of prayer by seeming to imply its futility, but should rather tend to purify and exalt the objects of prayer. The savage prays to his idol, that he may have success in killing his enemies. The Hottentot whips and worships his fetich in blind but eager quest of some sensual boon, that he may consume it upon his lusts. The prayers of the Vedic Books are the childish prayers of an unspiritual and childish people. "They pray," says Max Müller, "for the playthings of life, for houses and homes, for cows and horses, and they plainly tell the gods that if they will only be kind and gracious they will receive rich offerings in return." And do *we*, asks the critic of comparative religions, we Christians of this nineteenth century, "do we do much otherwise," if regard be had to the quality of *our* petitions? Prof. Henry held that it was both the duty and privilege of enlightened Christians to "do much otherwise," by praying preëminently, if not exclusively, for spiritual blessings. And hence he held that the highest natural philosophy combines with the highest Christian faith to transfer the religious thoughts, feelings, and aspirations of man more and more from things seen to things unseen, and from things temporal to things eternal. This view of his had nothing of quietism or of mysticism in it. Still less was it the expression of an apathetic stoicism. It was only the philosopher's way of praying to the great All-Father, in the spirit of St. Augustine, "*Da quod jubes, et jube quod vis.*"

I have made this reference to the opinions of Prof. Henry on the relations of science to religion, as also on the relations of natural philosophy to prayer, not only for the light they shed on the character of the man, but also for a reason which is peculiar to this Society, and which it may be a matter of interest for you to know. Immediately after his last unanimous election as the president of our Society, he communicated to me his purpose to make the relations of science and religion, as also the true import of prayer, the subject of his annual presidential address. He gave me an outline of the views he intended to submit, and I have here given but a brief *résumé* of them, according to my recollections of the colloquy, which was only one of many similar conferences previously had on the same high themes. He said that it would be, perhaps, the last time he should ever be called

to deliver a presidential address before the Society he so much loved, and that he wished to speak as became an humble patron of Science, believing fully in her high mission, and at the same time as an humble Christian, believing fully in the fundamental truths of Revelation. That he was not able to fulfil this purpose will be as much a source of regret to you as it is to me, but when I compare the valediction which it was in his heart to utter, with the peaceful end which came a few months later to crown his days with the halo of a finished life, I console myself with the thought that no last words of his were needed to seal on our hearts the lesson taught by his long and splendid career. Being dead he yet speaketh.

It is, indeed, the shadow of a great affliction which his death has cast upon our Society, but the light of his life pierces through the darkness, and irradiates for us all the paths of duty and labor, of honor and purity, of truth and righteousness, in which he walked with an eye that never blenched and a foot that never faltered. We shall not see his face any more, beaming with gladness and with the mild splendor of chastened intellect, but we shall feel his spiritual presence whenever we meet in this hall. We shall never hear his voice again, but its clear and gentle tones, as from yonder chair he expounded to us the mysteries of nature, will re-echo in the chambers of memory with only a deeper import, now that he has gone to join the "dead but sceptred sovereigns who still rule our spirits from their urns."

O

A MEMOIR OF JOSEPH HENRY.

A SKETCH OF HIS SCIENTIFIC WORK.*

By William B. Taylor.

To cherish with affectionate regard the memory of the venerated dead is not more grateful to the feelings, than to recall their excellences and to retrace the stages and occasions of their intellectual conquests is instructive to the reason. Few lives within the century are more worthy of admiration, more elevating in contemplation, or more entitled to commemoration, than that of our late most honored and beloved president—JOSEPH HENRY.

Distinguished by the extent of his varied and solid learning, possessing a wide range of mental activity, so great were his modesty and self-reserve, that only by the accidental call of occasion would even an intimate friend sometimes discover with surprise the fulness of his information and the soundness of his philosophy, in some quite unsuspected direction. Remarkable for his self-control, he was no less characterized by the absence of self-assertion. Ever warmly interested in the development and advancement of the young, he was a patient listener to the trials of the disappointed, and a faithful guide to the aspirations of the ambitious. Generous without ostentation, he was always ready to assist the deserving—by services—by counsel—by active exertions in their behalf.

In his own pursuits Truth was the supreme object of his regard,—the sole interest and incentive of his investigations; and in its prosecution he brought to bear in equable combination qualities of a high order; quickness and correctness of perception, inventive ingenuity in experimentation, logical precision in deduction, perseverance in exploration, sagacity in interpretation.†

* A large portion of the following discourse (including nearly the whole of the section on the "Administration of the Smithsonian Institution,") was necessarily omitted on the occasion of its delivery.

† Henry's tribute to Peltier, seems peculiarly applicable to himself. "He possessed in an eminent degree the mental characteristics necessary for a successful scientific discoverer; an imagination always active in suggesting hypotheses for the explanation of the phenomena under investigation, and a logical faculty never at fault in deducing consequences from the suggestions best calculated to bring them to the test of experi-

EARLY CAREER.

Of Henry's early struggles,—of the youthful traits which might afford us clue to his manhood's character and successes, we have but little preserved for the future biographer. Deprived of his father at an early age, he was the sole care and the sole comfort of his widowed mother. Carefully nurtured in the stringent principles of a devout religious faith, he adhered through life to the traditions and to the convictions derived from his honorable Scottish ancestry.

As a youth he was by no means precocious,—as seldom have been those who have left a permanent influence on their kind. He seems to have felt no fondness for his early schools, and to have shown no special aptitude for the instructions they afforded. Like many another unpromising lad, he followed pretty much his own devices, unconcerned as to the development of his latent capabilities. The books he craved were not the books his school-teachers set before him. The novel and the play interested and absorbed the active fancy naturally so exuberant in youth; and the indications from his impulsive temperament were that he would probably become a poet—a dramatist—or an actor.

He was however from his childhood's years a close observer—both of nature, and of the peculiarities of his fellows: and one characteristic early developed gave form and color to his mental disposition throughout later years,—an unflagging energy of purpose.

About the year 1814, while a boy of still indefinite aims and of almost as indefinite longings, having been confined to the house for a few days, in consequence of an accidental injury, his restless attention happened to be drawn to a small volume on Natural Philosophy, casually left lying on a table by a boarder in the house. Listlessly he opened it and read. Before he reached the third page, he became profoundly interested in the statement of some of the enigmas of the great sphinx—Nature. A new world seemed opening to his inquisitive eyes. Eagerly on he read,—intent to find the hidden meanings of phenomena which hitherto covered by the "veil of familiarity" had never excited a passing wonder or a doubting question. Was it possible ever to discover the real causes of things? Here was a new Ideal—if severer, yet grander than that of art. He no longer read with the languid enjoyment of a passive recipient; he felt the new necessity of reaching out with all the faculties of a thinker, with all the

ence; an invention ever fertile in devising apparatus and other means by which the test could be applied; and finally a moral constitution which sought only the discovery of truth, and could alone be satisfied with its attainment." (*Smithsonian Report* for 1867, p. 158.)

activity of a co-worker.* For the first time he realized (though with no conscious expression of the thought) that there is—so to speak,—an imagination of the intellect, as well as of the emotional soul;—that *Truth* has its palaces no less gorgeous—no less wonderful than those reared by fancy in homage to the *Beautiful.*

The owner of the book observing the close application of the boy, very kindly presented it to him;† and the thankful receiver many years afterward placed upon the inside of its front cover, the following memorandum:—

"This book although by no means a profound work, has under Providence exerted a remarkable influence on my life. It accidentally fell into my hands when I was about sixteen years old, and was the first book I ever read with attention. It opened to me a new world of thought and enjoyment; invested things before almost unnoticed, with the highest interest; fixed my mind on the study of nature; and caused me to resolve at the time of reading it, that I would immediately commence to devote my life to the acquisition of knowledge. J. H."

The new impulse was not a momentary fascination. Thenceforward the novel was thrown aside, and poesy neglected; though to his latest day a sterling poem never failed to strongly impress him. As it dawned upon his reason that the foundation of the coveted knowledge must be the studies he had thought so irksome, he at once determined to repair as far as possible his loss of time, (being then an apprentice to his cousin John F. Doty, a Watch-maker and Silver-smith in Albany,) by taking evening lessons from two of the Professors in the Albany Academy; applying himself diligently to geometry and mechanics. And here shone out that strength of will which enabled him to rise above the harassing obstacle of the *res angusta domi.* With the consent of his employer, so soon as he felt able, (although yet a mere boy,) he managed to procure a position as teacher in a country school, where for seven months successfully instructing boys not much younger than himself, in what he had acquired, he was enabled by rigid economy to take a regular course of instruction at the Albany Academy. Again returning to his school-teaching, he furnished himself with the means of completing his studies at the Academy; where learning that the most important key to the accurate knowledge of nature's laws is a

* "There is a great difference between *reading* and *study*, or between the indolent reception of knowledge without labor, and that effort of mind which is always necessary in order to secure an important truth and make it fully our own." J. Henry. (*Agricultural Report* of the Patent Office, for 1857, p. 421.)

† The title of this book is "Lectures on Experimental Philosophy, Astronomy, and Chemistry: by G. Gregory, D.D., Vicar of West-ham." 12mo. London, 1808. The owner of the book was a young Scotchman named Robert Boyle; who was one of the boarders at his mother's house in Albany, N. Y.

familiarity with the logical processes of the higher mathematics, he resolutely set himself to work to master the intricacies of the differential calculus.

Having finished his academic course and passed with honor through his examinations, he then through the warm recommendation of Dr. T. Romeyn Beck—the distinguished Principal of the Academy, obtained a position as private tutor in the family of General Stephen Van Rensselaer.* As this duty did not exact more than about three hours a day of his attendance, he applied his ample leisure (having in view the medical profession)—partly to the assistance of Dr. Beck in his chemical experiments, and partly to the study of Anatomy and Physiology, under Doctors Tully and Marsh.

His devotion to Natural Philosophy which had only grown and strengthened with his own growth in knowledge, led him constantly to repeat any unusual experiment as soon as reported in the foreign scientific journals; and to devise new modifications of the experiment for testing more fully the range and operation of its fundamental principles.

Communications to the Albany Institute.—The "Albany Institute" was organized May 5th 1824, by the union of two older Societies; with General Stephen Van Rensselaer as its President:† and young Henry became at once an active member: though with his modest estimate of his own attainments, he preferred the part of listener and acquirer, to that of seeming instructor, till urged by those who knew him best to add his contributions to the general garner.

Henry's first communication to the Institute was read October 30th 1824 (at the age of about twenty-six years) and was "On the chemical and mechanical effects of steam: with experiments designed to illustrate the great reduction of temperature in steam of high elasticity when suddenly expanded."‡ From the stopcock of a strongly made copper vessel in which steam could be safely generated under considerable pressure, he allowed an occasional escape; and he showed by holding the bulb of a thermometer in the jet of steam, at a fixed distance (say of four inches) from the orifice, that as the temperature and pressure increased within the boiler, the indications of the thermometer without grew lower;—the expansion and consequent cooling of the escap-

* Presiding Officer of the original Board of Trustees of the Albany Academy.

† The Albany Institute resulted from the fusion of "The Society for the Promotion of Useful Arts in the State of New York," organized Feb. 1791 (incorporated April 2nd 1804,) and the "Albany Lyceum of Natural History" formed and incorporated April 23rd 1823: of which latter society, Henry had been a member.

‡ *Trans. Albany Inst.* vol. i. part 2, p. 30.

ing steam under great pressure, increasing in a higher ratio than the increased temperature required for the pressure. And finally he exhibited the striking paradox, that the jet of saturated steam from a boiler will not scald the hand exposed to it, at a prescribed near distance from the try-cock, provided the steam be sufficiently hot.*

Prolific and skilful in devising experiments, Henry delighted in making evident to the senses the principles he wished to impress upon the mind. Extending the law of cooling by expansion, from steam at high temperatures, to air at ordinary temperatures, his next communication to the Institute (made March 2nd 1825,) was "On the Production of Cold by the Rarefaction of Air." As before, he accompanied his remarks by several characteristic exhibitions.

"One of these experiments most strikingly illustrated the great reduction of temperature which takes place on the sudden rarefaction of condensed air. Half a pint of water was poured into a strong copper vessel of a globular form, and having a capacity of five gallons; a tube of one-fourth of an inch caliber with a number of holes near the lower end, and a stop-cock attached to the other extremity, was firmly screwed into the neck of the vessel; the lower end of the tube dipped into the water, but a number of the holes were above the surface of the liquid, so that a jet of air mingled with water might be thrown from the fountain. The apparatus was then charged with condensed air, by means of a powerful condensing pump, until the pressure was estimated at nine atmospheres. During the condensation the vessel became sensibly warm. After suffering the apparatus to cool down to the temperature of the room, the stop-cock was opened: the air rushed out with great violence, carrying with it a quantity of water, which was instantly converted into snow. After a few seconds, the tube became filled with ice, which almost entirely stopped the current of air. The neck of the vessel was then partially unscrewed, so as to allow the condensed air to rush out around the sides of the screw: in this state the temperature of the whole interior atmosphere was so much reduced as to freeze the remaining water in the vessel."†

Although the principle on which this striking result was based was not at that time new, it must be borne in mind that this

* While it requires a heat of 250° F. to generate a steam-pressure of two atmospheres (*i. e.* one additional to the existing), 25° higher will produce a pressure of three atmospheres, and 100° higher, (or 355° F.) will produce a pressure of nine atmospheres: the curve (by rectangular co-ordinates of temperature and pressure) resembling a hyperbola. The increased velocity at high pressure produces a molecular *momentum* of expansion carrying the rarefaction beyond the limit of atmospheric pressure; and in the case of the exposed hand, the injected air current doubtless adds to the cooling impression.

† *Trans. Albany Inst.* vol. i. part 2, p. 36.

particular application, thus publicly exhibited, was long before any of the numerous patents were obtained for ice-making, not a few of which adopted substantially the same process.

State Appointment as a Civil Engineer.—Through the friendship and confidence of an influential judge, Henry received about this time an unexpected offer of an appointment as Engineer on the survey of a route for a road through the State of New York, from the Hudson river on the east, to lake Erie on the west. The proposal was too tempting to his natural proclivities to be refused; and being appointed, he embarked upon his new and arduous duties with the zeal and energy which were so prominent a feature of his character. He completed the survey with credit to himself, and to the entire satisfaction of the Commissioners of the work.*

So attractive appeared the profession of engineer to his enterprising disposition, that he was about to accept the directorship in the construction of a canal in Ohio, when he was informed that the Chair of Mathematics in the Albany Academy would soon become vacant, and that his own name had already been prominently brought forward in connection with the position. At the urgent solicitation of his old friend and former teacher Dr. T. Romeyn Beck, he consented with some hesitation to signify his willingness to accept the vacant chair if appointed thereto.

Election as Professor of Mathematics.—In the spring of 1826, Henry was duly elected by the Trustees of the Albany Academy to the Professorship of Mathematics and Natural Philosophy in that Institution. As the duties of his office did not commence till September of that year, he was allowed a practical vacation of about five months; which was partly occupied with a geological exploration in the adjoining counties, as assistant to Professor Eaton, of the Rensselaer School, and partly devoted to a conscientious preparation for his new position.

In a worldly point of view, this variety of occupation and versatility of adaptation might perhaps be regarded as unfavorable to success. As a method of culture, it was of unquestionable advantage to his intellectual powers. A hard student, with great capacity for close application, he accumulated large stores of information: and in addition to his constant thirst for acquirement in different directions, his leisure was occupied to a considerable extent with physical and chemical examinations. On the 21st of March 1827, he delivered before the Albany Institute a lecture on "Flame," accompanied with experiments.†

* In a popular journal ("The Eclectic Magazine") it is stated: "His labors in this work were exceedingly arduous and responsible. They extended far into the winter, and the operations were carried on in some instances amid deep snows in primeval forests."

† *Trans. Albany Inst.* vol. i. part 2, p. 59.

Meteorological Work.—The Regents of the University of the State of New York, endowed by the State Legislature with supervisory functions over the public educational institutions of the State, in 1825 established a system of meteorological observation for the State, by supplying to each of the Academies incorporated by them, a thermometer and a rain-gauge, and requiring them to keep a daily register of prescribed form, to entitle them to their portion of the literature fund of the State. In 1827, the Hon. Simeon De Witt, Chancellor of the Board of Regents, associated with himself Dr. T. Romeyn Beck and Professor Henry of the Albany Academy, to prepare and tabulate the results of these observations. The first Abstract of these collections (for the year 1828) comprised tabulations of the monthly and yearly means of temperature, wind, rain, etc., at all the stations, an account of meteorological incidents generally, and a table of "Miscellaneous Observations" on the dates of notable phases of organic phenomena connected with climatic conditions. These annual Abstracts, to which Henry devoted a considerable share of his attention, were continued through a series of years and were published in the "Annual Reports of the Regents of the University to the Legislature of the State of New York.* The third Abstract (for 1830) includes an accurate tabulation by Henry of the Latitudes, Longitudes, and Elevations of all the meteorological stations; over forty in number.

ELECTRICAL RESEARCHES AT ALBANY; FROM 1827-1835.

Of Henry's distinguished success as a lecturer and teacher, in imparting to his pupils a portion of his own zeal and earnestness in the pursuit of scientific knowledge, as well as in winning their affection and in inspiring their esteem, it is not designed here to discourse; but rather of his solitary labors outside of his professional occupation in communicating and diffusing knowledge. Very shortly after his occupation of the academic chair of mathematics and physics, he turned his attention to the experimental study of that mysterious agency—electricity. Professor Schweigger of Halle, had improved on Œrsted's galvanic indicator (of a single wire circuit) by giving the insulated wire a number of turns around an elongated frame longitudinally enclosing the compass needle, and by thus multiplying the effect of the galvanic circuits, had converted it into a real *measuring* instrument—a "galvanometer."† Ampère and Arago of Paris, develop-

* *Reports of Regents*, &c. Albany, vol. i. 1829-1835.

† The name of Galvani (as original discoverer of chemico-electricity) is usually retained to design te both the current and its generator; although the chemico-electric pile and battery were really first con rived by Volta in 1800. In the same manner Oersted is generally accounted the discoverer of electro-magnetism, although he never devised an electro-

ing Œrsted's announcement of the torsional or equatorial reaction between a galvanic conductor and a magnetic needle, had found that a circulating galvanic current was capable not only of deflecting a suspended magnet, but of *generating* magnetism—permanently in sewing needles, and temporarily in pieces of iron wire, when placed within a glass tube around which the conjunctive wire of the battery had been wound in a loose helix; and had thus created the "electro-magnet."* The scientific world was just aroused to the close interrogation of this new marvel, each questioner eager to ascertain its most efficient conditions, and to increase its manifestations. William Sturgeon of Woolwich, England, had extended the discoveries of Ampère and Arago, by dispensing with the glass tube, constructing a "horse-shoe" bar of soft iron (after the form of the usual permanent magnet) coated with a non-conducting substance, and winding the copper conjunctive wire directly upon the horse-shoe; and had thus produced the first *efficient* electro-magnet;—capable of sustaining several pounds by its armature, when duly excited by the galvanic current. He had also greatly improved lecture-room apparatus for illustrating the electro-magnetic reactions of rotations, etc., (where a permanent magnet is employed) by introducing stronger magnets, and thereby succeeding in exhibiting the phenomena on a larger scale, with a considerable reduction of the battery power.†

Faraday had not yet commenced the series of researches which in after years so illumined his name, when Henry published his first contribution to electrical science, in a communication read before the Albany Institute, October 10th, 1827, "On some Modifications of the Electro-Magnetic Apparatus." From his experimental investigations he was enabled to exhibit all the class illustrations attempted by Sturgeon, on even a still larger and more conspicuous scale, with the employment of very weak magnets (where required), and with a still further considerable reduction of the battery power. These quite striking and unexpected results were obtained by the simple expedient of adopting in every case where single circuits had previously been used, the manifold coil of fine wire which Schweigger had employed to increase the sensibility of the galvanometer. He remarks:—

magnet; and appears not to have been the first even to discover the directive influence of a current on a magnetic needle.

* *Annales de Chimie et de Physique*, 1820, vol. xv. pp. 93–100.

† *Trans. Soc. Encouragement Arts*, etc., 1825, vol. xliii. pp. 38–52. This battery (of a single element) consisted "of two fixed hollow concentric cylinders of thin copper, having a movable cylinder of zinc placed between them. Its superficial area is only 130 square inches, and it weighs no more than 1 lb. 5 ozs." Mr. Sturgeon was deservedly awarded the Silver Medal of the Society for the Encouragement of Arts etc., "for his improved electro-magnetic apparatus." Described also in Thomson's *Annals of Philos.*, Nov. 1826, vol. xii., new series, pp. 357–361.

"Mr. Sturgeon of Woolwich, who has been perhaps the most successful in these improvements, has shown that a strong galvanic power is not essentially necessary even to exhibit the experiments on the largest scale. . . . Mr. Sturgeon's suite of apparatus, though superior to any other as far as it goes, does not however form a complete set: as indeed it is plain that his principle of strong magnets cannot be introduced into every article required, and particularly into those intended to exhibit the action of the earth's magnetism on a galvanic current, or the operation of two conjunctive wires on each other. To form therefore a set of instruments on a large scale that will illustrate all the facts belonging to this science, with the least expense of galvanism, evidently requires some additional modification of apparatus, and particularly in those cases in which powerful magnets cannot be applied. And such a modification appears to me to be obviously pointed out in the construction of Professor Schweigger's Galvanic Multiplier: the principles of this instrument being directly applicable to all the experiments in which Mr. Sturgeon's improvement fails to be useful." *

The coils employed in the various articles of apparatus thus improved, comprised usually about twenty turns of fine copper wire wound with silk to prevent metallic contact, the whole being closely bound together. To exhibit for example Ampère's ingenious and delicate experiment showing the directive action of the earth as a magnet on a galvanic current when its conductor is free to move, (usually a small wire frame with its extremities dipping either into mercury cups, or into mercury channels,) or its simpler modification, the "ring" of De La Rive (usually an inch or two in diameter and made to freely float with its galvanic element in its own bath,) the effect was strikingly enhanced by Henry's method of suspending by a silk thread a large circular coil twenty inches in diameter, of many wire circuits bound together with ribbon,—the extremities of the wire protruding at the lower part of the hoop, and soldered to a pair of small galvanic plates;—when by simply placing a tumbler of acidulated water beneath, the hoop at once assumed (with a few oscillations) its equatorial position transverse to the magnetic meridian. By a similar arrangement of two circular coils of different diameters, one suspended within the other, Ampère's fine discovery of the mutual action of two electric currents on each other, was as strikingly displayed. Such was the character of demonstration by which the new Professor was accustomed to make visible to his classes the principles of electro-magnetism: and it is safe to say that in simplicity, distinctness, and efficiency, such apparatus for the lecture-room was far superior to any of the kind then existing.

* *Trans. Albany Institute*, vol. i. pp. 22, 23.

Should any one be disposed to conclude that this simple extension of Schweigger's multiple coil was unimportant and unmeritorious, the ready answer occurs, that talented and skilful electricians, laboring to attain the result, had for six years failed to make such an extension. Nor was the result by any means antecedently assured by Schweigger's success with the galvanometer. If Sturgeon's improvement of economizing the battery size and consumption, by increasing the magnet factor (in those few cases where available), was well deserving of reward, surely Henry's improvement of a far greater economy, by increasing the circuit factor (entirely neglected by Sturgeon) deserved a still higher applause.

In a subsequent communication to Silliman's Journal, Henry remarks on the results announced in October, 1827:—"Shortly after the publication mentioned, several other applications of the coil, besides those described in that paper, were made in order to increase the size of electro-magnetic apparatus, and to diminish the necessary galvanic power. The most interesting of these was its application to a development of magnetism in soft iron, much more extensive than to my knowledge had been previously effected by a small galvanic element." The electro-magnet figured and described by Sturgeon, (in his communication of November, 1825,) consisted of a small bar or stout iron wire bent into a U or horse-shoe form, having a copper wire wound loosely around it in eighteen turns, with the ends of the wire dipping into mercury cups connected with the respective poles of a battery having 130 square inches of active surface. This was undoubtedly the most efficient electro-magnet then in existence.

In June of 1828, Henry exhibited to the Albany Institute a small-sized electro-magnet closely wound with silk-covered copper wire about one-thirtieth of an inch in diameter. By thus insulating the conducting wire instead of the magnetic bar or core, he was enabled to employ a compact coil in close juxtaposition from one end of the horse-shoe to the other, obtaining thereby a much larger number of circuits, and having each circuit more nearly at right angles with the magnetic axis. The lifting power of this magnet is not stated, though it must obviously have been much more powerful than the one described by Sturgeon.

In March of 1829, Henry exhibited to the Institute a somewhat larger magnet, of the same character. "A round piece of iron about one quarter of an inch in diameter, was bent into the usual form of a horse-shoe, and instead of loosely coiling around it a few feet of wire, as is usually described, it was tightly wound with 35 feet of wire covered with silk, so as to form about 400 turns: a pair of small galvanic plates which could be dipped into a tumbler of diluted acid, was soldered to the ends of the wire, and the whole mounted on a stand. With these small

plates, the horse-shoe became much more powerfully magnetic than another of the same size (and wound in the usual manner) by the application of a battery composed of 28 plates of copper and zinc each 8 inches square." In this case the coil was wound upon itself.

Henry's "Quantity" Magnet compared with Moll's.—Shortly after this, Dr. G. Moll, (Professor of Natural Philosophy in the University of Utrecht,) having seen in England, in 1828, an electro-magnet of Sturgeon's which supported nine pounds from its armature, "determined to try the effect of a larger galvanic apparatus;" and in a paper published in 1830,* remarks: "I obtained results which appear astonishing, and are—as far as the intensity of magnetic force is concerned, altogether new. I have anxiously looked since that time into different scientific continental and English journals, without finding any further attempt to extend and improve Mr. Sturgeon's original experiment." Moll's horse-shoe formed of a round bar of iron about 1 inch thick, was about 8½ inches in height, and had a wrapped copper wire of about one-eighth inch diameter coiled 83 times around it. The weight of the horse-shoe and wire was about 5 pounds; of the armature, about 1¼ pound; and with a single galvanic pair whose acting zinc surface was about 11 square feet, the electro-magnet supported about 50 pounds. With cautious additions, the load could be increased to 75 pounds. An additional galvanic pair of about 6 square feet was applied without increasing the power of the magnet.†

As soon as the account of Moll's magnet reached this country, Henry who had obtained and had publicly exhibited nearly two years previously, considerably higher results, and who realized that there was at least one very important difference of construction between his own magnet and that of the Dutch savant, felt it a duty at once to publish the details of his own researches, in a more public form:—which he accordingly did in the January number of Silliman's American Journal of Science for 1831; (then published only quarterly;) causing a copy of Professor Moll's paper, taken from Brewster's Edinburgh Journal of Science for October 1830, to be inserted in the same number. At the conclusion of his own article he remarks: "The only effect Professor Moll's paper has had over these investigations, has been to hasten their publication: the principle on which they were instituted was known to us nearly two years since, and at that time exhibited to the Albany Institute."

The magnet which he had subsequently made, consisted of a cylindrical bar of iron one-half inch in diameter and about 10 inches long, bent into a horse-shoe and closely wound with several

* *Bibliothèque Universelle*, 1830, cah. 45, p. 19.

† Brewster's *Edinburgh Jour. Sci* Oct. 1830, vol. iii. n. s. pp. 209-218.

strands of fine silk-covered wire, each about 30 feet long; which arrangement when placed in the circuit of a single galvanic pair whose zinc surface was 6 inches by 4 inches (one-sixth of a square foot) sustained by its armature "39 pounds, or more than fifty times its own weight;" Moll's highest result (of which he justly felt proud) being only fifteen times the weight of the magnet, with 11 square feet of zinc surface. While Henry's magnet had the practical advantage of being about only one-half the size of Moll's—in each dimension, (and therefore about only one-eighth its weight without wrappings,) yet it supported more than half his load: (39 pounds to 75 pounds.) Moll had employed a single copper wire one-eighth inch thick and about 22 feet long: Henry, several strands each about one thirty-sixth of an inch thick, and 30 feet long;—the former making 83 turns around the iron core,—the latter, several hundred turns. But the most surprising contrast resulting from these differences was the enormous difference of battery-power applied; Moll pushing his up to 17 square feet,—Henry reducing his to one-sixth of one square foot. With a galvanic element reduced to two and a half square inches, his magnet sustained 28 pounds; or more than double the relative duty of Moll's at its highest power. The philosopher of Utrecht, though he evidently realized with him of Albany, the importance of close winding, employed but a single layer of coil. The latter, by means of well-considered trials had ascertained the great increase of magnetic force resulting from a succession of coils.

To Henry therefore belongs the exclusive credit of having first constructed the magnetic "spool" or "bobbin": that form of coil since universally employed for every application of electro-magnetism, of induction, or of magneto-electrics. This was his first great contribution to the science and to the art of galvanic magnetization. It may be very confidently affirmed that prior to 1829, no one on either hemisphere had ever thought of winding the legs of an electro-magnet on the principle of the "bobbin"; and that not till after the publication of Henry's method in January of 1831, was it ever employed by any European physicist.

"These experiments conclusively proved that a great development of magnetism could be effected by a very small galvanic element, and also that the power of the coil was materially increased by multiplying the number of wires, without increasing the length of each. The multiplication of the wires increases the power in two ways; first, by conducting a greater quantity of galvanism, and secondly, by giving it a more proper direction; for since the action of a galvanic current is directly at right angles to the axis of a magnetic needle,—by using several shorter wires, we can wind one on each inch of the length of the

bar to be magnetized, so that the magnetism of each inch will be developed by a separate wire: In this way the action of each particular coil becomes directed very nearly at right angles to the axis of the bar, and consequently the effect is the greatest possible. This principle is of much greater importance when large bars are used. The advantage of a greater conducting power from using several wires might in a less degree be obtained by substituting for them one large wire of equal sectional area; but in this case the obliquity of the spiral would be much greater, and consequently the magnetic action less."* Moll's single conducting wire of one eighth inch diameter, while therefore *electrically* equivalent to about 20 of Henry's conducting wires (of the same length and weight) would be *magnetically* inferior thereto—for equal iron cores.

Notwithstanding that Henry's successes were thus both earlier and more brilliant than those of Moll, the two names are usually associated together by European writers in treating of the development of the magnet.†

Among the subsequent experiments on which Henry was engaged at the time of receiving the Edinburgh Journal of Science containing Moll's paper, was a series on a much larger magnet, consisting of a bar of soft iron two inches square (with the angles rounded) and twenty inches long, bent into a horse-shoe about nine inches high, and weighing 21 pounds. Its armature—a piece from the same bar ground to fit truly the ends of the horse-shoe, weighed 7 pounds. Nine coils of copper bell-wire each 60 feet in length (making 540 feet in all) were separately wound on different portions of the horse-shoe. "These coils were not continued around the whole length of the bar, but each strand of wire according to the principle before mentioned, occupied about two inches, and was coiled several times backward and forward over itself: the several ends of the wire were left projecting and all numbered, so that the first and last end of each strand might be readily distinguished. In this manner was formed an experimental magnet on a large scale, with which several combinations of wire could be made by merely uniting the different projecting ends. Thus if the second end of the first wire be soldered to the first end of the second wire, and so on

* Sill. *Am. Jour. Sci.* January, 1831, vol. xix. p. 402. The three names—Arago, Sturgeon, and Henry, may well typify the infancy, the youth, and the mature manhood of the electro-magnet.

† Faraday in subsequently investigating the conditions of galvanic induction, referred with approbation to the magnets of Moll and Henry as best calculated to produce the effects sought. In constructing his duplex helices for observing the direction of the induced current, he however adopted Henry's method by winding 12 coils of copper wire each 27 feet long—one upon the other. (*Phil. Trans. Roy. Soc.* Nov. 24, 1831, vol. cxxii. (for 1832,) pp. 126, and 138. *Experimental Researches*, etc., vol. i. art. 6, p. 2; and art. 57, p. 15.)

through all the series, the whole will form a continued coil of one long wire. By soldering different ends the whole may be formed into a double coil of half the length, or into a triple coil of one-third the length, etc. The horse-shoe was suspended in a rectangular wooden frame 3 feet 9 inches high and 20 inches wide."

Two of the wires (one from each extremity of the legs) being joined together by soldering, so as to form a single circuit of 120 feet, with its extreme ends connected with the battery, produced a lifting power of 60 pounds. (*Ex.* 19.) The same two wires being separately connected with the same battery (forming a double circuit of 60 feet each) a lifting power of 200 pounds was obtained: (*Ex.* 10.) or more than three times the power of the former case with the same wire. Four wires (two from each extremity of the legs) being separately connected with the battery, (forming four circuits,) gave a lifting power of 500 pounds. (*Ex.* 12.) Six wires (three from each leg) united in three pairs, (forming three circuits of 120 feet each,) gave a lifting power of 290 pounds. (*Ex.* 18.) The same six wires being separately connected with the battery in six independent circuits, produced a lifting power of 570 pounds: (*Ex.* 13.) or very nearly double that of the same wires in double lengths. When all the nine wires were separately attached to the battery, a lifting power of 650 pounds was evoked. (*Ex.* 14.) In all these experiments "a small single battery was used consisting of two concentric copper cylinders, with zinc between them: the whole amount of zinc surface exposed to the acid from both sides of the zinc was two-fifths of a square foot: the battery required only half a pint of dilute acid for its submersion."

"In order to ascertain the effect of a very small galvanic element on this large quantity of iron, a pair of plates *exactly one inch square*, was attached to all the wires: the weight lifted was 85 pounds." (*Ex.* 16.) This was certainly a very remarkable result; particularly when compared with Moll's 75 pounds with *eleven square feet* of zinc. In order to obtain the maximum attractive power of this magnet, with its nine coils, "a small battery formed with a plate of zinc 12 inches long and 6 wide, and surrounded by copper, was substituted for the galvanic element used in the former experiments: the weight lifted in this case was 750 pounds." (*Ex.* 15.) This is exactly ten times the maximum weight supported by Moll's magnet with a far greater battery power. In illustration of the feeble power of the magnetic poles when exerted separately, it was found that with precisely the same arrangements giving a holding power of 750 pounds to the double contact armature,—either pole alone was capable of sustaining only 5 or 6 pounds: "and in this case we never succeeded in making it lift the armature—weighing 7

pounds. We have never seen the circumstance noticed of so great a difference between a single pole and both."

Henry's "Intensity" Magnet.—But Henry's remarkable paper of January 1831 contains still another original contribution to the theory and practice of electro-magnetics, no less important than his invention of the magnetic spool. While Moll had endeavored to induce strong magnetism by the use of a powerful "quantity" battery, Henry had labored to derive from a minimum galvanic power its maximum magnetizing effect: and in his varied experiments on these two factors, he discovered very curious and unsuspected relations between them. A great majority of investigators—after having definitely ascertained the striking fact of the great inferiority in magnetizing power, of a single long continuous coil, to a proportionally shortened circuit of multiple coils,—would naturally have been led to abandon all further investigation of the feebler system. Henry however recognized in this a field of instructive inquiry: and for the first time showed that the coil of short and numerous circuits, least affected by a battery of many pairs, was on the contrary most responsive to a single galvanic element; while the single extended coil, least influenced by a single pair, was most excited by a battery of elements. He appears to have been the first to form a clear conception of the difference between "intensity" and "quantity" both in the battery and in the magnet: a difference which (as referred to the current), he was accustomed figuratively to illustrate by the mechanical difference between equal momentums of high and low velocity.*

The illustrious Laplace had suggested to Ampère in 1820,—immediately upon the discovery of the galvanometer, that by sending the galvanic current through long wires connecting two distant stations, the deflections of enclosed magnetic needles would constitute very simple and efficient signals for an instantaneous telegraph.† Peter Barlow the eminent English mathematician and magnetician taking up the suggestion, had endeavored more fully to test its practicability. He has thus

* "In describing the results of my experiments, the terms 'intensity' and 'quantity' magnets were introduced to avoid circumlocution, and were intended to be used merely in a technical sense. By the *intensity* magnet I designated a piece of soft iron so surrounded with wire that its magnetic power could be called into operation by an 'intensity' battery; and by a *quantity* magnet, a piece of iron so surrounded by a number of separate coils that its magnetism could be fully developed by a 'quantity' battery." (*Smithsonian Report* for 1857, p. 103.) Although these terms are somewhat antiquated, and repudiated by recent writers, they will be retained in this Memoir, for their convenience.

† *Annales de Chimie et de Physique*, 1820, vol. xv. pp. 72, 73. Ampère made the experiment suggested by Laplace, through a long conducting wire "with perfect success." The length of the wire is not stated.

stated the result: "In a very early stage of electro-magnetic experiments it had been suggested that an instantaneous telegraph might be established by means of conducting wires and compasses. The details of this contrivance are so obvious, and the principle on which it is founded so well understood, that there was only one question which could render the result doubtful; and this was,—is there any diminution of effect by lengthening the conducting wire? It had been said that the electric fluid from a common [tin-foil] electrical battery had been transmitted through a wire four miles in length without any sensible diminution of effect, and to every appearance instantaneously; and if this should be found to be the case with the galvanic circuit, then no question could be entertained of the practicability and utility of the suggestion above adverted to. I was therefore induced to make the trial; but I found such a sensible diminution with only 200 feet of wire, as at once to convince me of the impracticability of the scheme. It led me however to an inquiry as to the cause of this diminution, and the laws by which it is governed."*

Henry in his researches just referred to, (assisted by his friend Dr. Ten-Eyck,) employed a small electro-magnet of one quarter inch iron "wound with about 8 feet of copper wire." Excited with a single pair "composed of a piece of zinc plate 4 inches by 7, surrounded with copper," (about 56 square inches of zinc surface,) the magnet sustained four pounds and a half. With about 500 feet of insulated copper wire (.045 of an inch in diameter) interposed between the battery and the magnet, its lifting power was reduced to two ounces;—or about 36 times. With double this length of wire, or a little over 1000 feet, interposed, the lifting power of the magnet was only half an ounce: thus fully confirming the results obtained by Barlow. With a small galvanic pair 2 inches square, acting through the same length of wire (over 1000 feet,) "the magnetism was scarcely observable in the horse-shoe." Employing next a trough battery of 25 pairs, having the same zinc surface as previously, the magnet in direct connection, (which before had supported four and a half pounds,) now lifted but seven ounces;—not quite half a pound. But with the 1060 feet of copper wire (a little more

* "On the Laws of Electro-magnetic Action." *Edinburgh Philosoph. Jour.* Jan. 1825, vol. xii. pp. 105–113. In explanation and justification of this discouraging judgment from so high an authority in magnetics, it must be remembered that both in the galvanometer and in the electro-magnet, the coil best calculated to produce large effects, was that of least resistance; which unfortunately was not that best adapted to a long circuit. On the other hand, the most efficient magnet or galvanometer was not found to be improved in result by increasing the number of galvanic elements. Barlow in his inquiry as to the "law of diminution" was led (erroneously) to regard the resistance of the conducting wire as increasing in the ratio of *the square root* of its length. (p. 111.)

than one-fifth of a mile) suspended several times across the large room of the Academy, and placed in the galvanic circuit, the same magnet sustained eight ounces: that is to say, the current from the galvanic trough produced greater magnetic effect after traversing this length of wire, than it did without it.

Speculating on this remarkable, and at the time, paradoxical result, Henry suggests in explanation, that "a current from a trough possesses more 'projectile' force (to use Professor Hare's expression,) and approximates somewhat in 'intensity' to the electricity from the common machine. May it not also be a fact that the galvanic fluid in order to produce the greatest magnetic effect should move with a smaller velocity, and that in passing through one-fifth of a mile, its velocity is so retarded as to produce a greater magnetic action? But be this as it may, the fact that the magnetic action of a current from a *trough* is at least not sensibly diminished by passing through a long wire, is directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph; * and it is also of material consequence in the construction of the galvanic coil From these experiments it is evident that in forming the coil we may either use one very long wire, or several shorter ones, as the circumstances may require: in the first case, our galvanic combinations must consist of a number of plates so as to give 'projectile' force; in the second, it must be formed of a single pair." †

Here for the first time is presented to science the "intensity" coil,—a spool of a single fine wire closely wound again and again upon itself,—with its singular capabilities—not of power, but (what was never before suspected nor imagined) of subtile excitation from a distant source. Here for the first time is established the important principle, that there must be a proportion between the aggregate internal resistance of the battery, and the whole external resistance of the conjunctive wire or conducting circuit; that for a "quantity" magnet of multiple coils (or their equivalent a large wire of corresponding weight) a "quantity" battery of surface, or a single galvanic element is required; while for an "intensity" magnet of extended continuous fine coil, an "intensity" battery of many small pairs is requisite: ‡ with the further discovery that the electro-motive force of the latter form enables a very long conductor to be employed without sen-

* Really Laplace's project;—not Barlow's.

† Silliman's *Am. Jour. Sci.* Jan. 1831, vol. xix. pp. 403, 404.

‡ "For circuits of small resistance, galvanometers of small resistance must be used. For circuits of large resistance, galvanometers of large resistance must also be used; not that their resistance is any advantage, but because we cannot have a galvanometer adapted to indicate very small currents without having a very large number of turns in the coil, and this involves necessarily a large resistance." Prof. F. Jenkin, *Electricity and Magnetism*, 12mo. London, and N. Y. 1873, chap. iv. sect. 8, p. 89.

sible diminution of the effect.* Professor Moll, the foremost of Europeans in the chase, and close upon the heels of Henry in one portion of his researches, produced a powerful "quantity" magnet, but one hopelessly and radically incapacitated from any such application.

These memorable consequences of careful and judicious experiment, carried on in 1829, and 1830, formed truly a most pregnant epoch in the history of the infant science; and constituted a valuable addition to the world's capital. Their adoption underlies all subsequent applications of the intermittent magnet, and is the indispensable basis of every form of the electro-magnetic telegraph since invented. They settled satisfactorily (in Barlow's phrase)—the "only question which could render the result doubtful:" and though derived from the magnet, were obviously as applicable to the galvanometer needle.

It is idle to say in disparagement of these successes, that in the competitive race of numerous distinguished investigators in the field, diligently searching into the conditions of the new found agency, the same results would soon have been reached by others. For of what discovery or invention may not the same be said? Only those who have sought in the twilight of uncertainty, can appreciate the vast economy of effort, by prompt directions to the path from one who has gained an advance. Not for what might be, but for the actual bestowal, does he who first grasps a useful truth, merit the return of at least a grateful recognition.

If these results apparently so simple when announced by Henry, have never been justly appreciated either at home or abroad, no such complaint ever escaped their author. No such thought seems ever to have occurred to his artless nature. For him the one sufficient incentive and recompense was the advancement of himself and others in the knowledge of nature's laws. With the telegraph consciously within his grasp, he was well content to leave to others the glory and the emoluments of its realization.

In the year 1831, Henry had suspended around the walls of one of the upper rooms in the Albany Academy, a mile of copper bell-wire interposed in a circuit between a small Cruickshank battery and an "intensity" magnet of continuous fine coil. A narrow steel rod—a permanent magnet—pivoted to swing horizontally like the compass needle, was arranged so that normally when pointing north, this end remained in contact with one leg of the soft iron core, while near the opposite end of the compass needle, a small stationary office-bell was placed. At each exci-

* Beyond a certain maximum length, there is of course a decrease of power proportioned to the increased resistance of a long conductor: but the magnetizing effect has not been found to be diminished in the ratio of its length.

tation of the electro-magnet, the compass needle was repelled from one leg (by its similar magnetism) and attracted by the other leg, so that its free end tapped the bell. This simple device the Professor was accustomed to exhibit to his classes, in illustration of the facility of transmitting signals to a distance by the swift action of electro-magnetism.

Henry regarded his "quantity" magnet as being *scientifically* more important than his "intensity" magnet; and his success in constructing such, of almost incredible power, caused numerous requisitions on his skill. In April, 1831, Professor Silliman published in his Journal "An Account of a large Electro-Magnet made for the Laboratory of Yale College," under his charge. The iron horse-shoe about one foot high was made from a three inch octagonal bar 30 inches long; and was wrapped with 26 strands of copper wire each about 28 feet long. When duly excited by a single galvanic element consisting of concentric cylinders of copper and zinc, presenting about five square feet of active surface, the magnet lifted more than a ton weight. For reversing the polarity of the magnet, a duplicate battery was oppositely connected with extensions of the ends of the coils, so that either battery could be alternately dipped. With a load of 56 pounds suspended from the armature, the poles of the magnet could be so rapidly reversed, that the weight would not fall. Professor Silliman remarks of the maker: "He has the honor of having constructed by far the most powerful magnets that have been known; and his last, weighing (armature and all) but 82½ pounds, sustains over a ton;—which is eight times more powerful than any magnet hitherto known in Europe."* And Sturgeon (the true foster-father of the magnet) thus heralds the Yale College triumph: "By dividing about 800 feet of conducting wire into 26 strands and forming it into as many separate coils around a bar of soft iron about 60 pounds in weight and properly bent into a horse-shoe form, Professor Henry has been enabled to produce a magnetic force which completely eclipses every other in the whole annals of magnetism; and no parallel is to be found since the miraculous suspension of the celebrated oriental imposter in his iron coffin."†

The first Electro-magnetic Engine.—Among his ingenious applications of the new power, Henry's invention of the Electro-magnetic Engine should here be noticed. In a letter to

* Silliman's *Am. Jour. Sci.* April, 1831, vol. xx. p. 201. *Relatively*, some of Henry's smaller magnets were many times more powerful than this. A miniature one made by Dr. Ten-Eyck under his direction, sustained 200 times its own weight; and one still smaller, sustained more than 400 times its own weight! (Sill. *Am. Jour. Sci.* vol. xix. p. 407.)

† *Philosoph. Magazine; and Annals*, 1832, vol. xi. p. 199.

his friend Professor Silliman, he says: "I have lately succeeded in producing motion in a little machine, by a power which I believe has never before been applied in mechanics,—by magnetic attraction and repulsion." The device consisted of a horizontal soft iron bar, about seven inches long, pivoted at its middle to oscillate vertically, and closely wrapped with three strands of insulated copper wire, whose ends were made by suitable extensions to project and bend downward at either end of the beam in reversed pairs, so as conveniently to dip into mercury thimbles in connection with the plates of the battery. Two upright permanent magnets having the same polarity, were secured immediately under the two ends of the oscillating bar, but separated from them by about an inch. So soon as the circuit was completed by the depression of one end of the oscillating electro-magnetic bar, a repulsion at this end co-operating with an attraction at the opposite end, caused immediately a contrary dip of the bar, which by reversing the polarity of this magnetic beam, thus produced a constant reciprocating action and movement. The engine beam oscillated at the rate of 75 vibrations per minute for more than an hour, or as long as the battery current was maintained.* This simple but original device comprised the first automatic pole-changer or commutator ever applied to the galvanic battery,—an essential element not merely in every variety of the electro-magnetic machine, but in every variety of the magneto-electric apparatus, and in every variety of the highly useful induction apparatus.

In an interesting "Historical Sketch of the rise and progress of Electro-magnetic Engines for propelling machinery," by the distinguished philosopher James P. Joule, he remarks: "Mr. Sturgeon's discovery of magnetizing bars of soft iron to a considerable power, and rapidly changing their polarity by miniature voltaic batteries, and the subsequent improved plan by Professor Henry of *raising* the magnetic action of soft iron,—developed new and inexhaustible sources of force which appeared easily and extensively available as a mechanical agent; and it is to the ingenious American philosopher above named, that we are indebted for the first form of a working model of an engine upon the principle of reciprocating polarity of soft iron by electro-dynamic agency "†

In Henry's deliberate contemplation of his own achievement, his remarkable sagacity and sobriety of judgment were conspicuously displayed. Unperturbed by the enthusiasm so natu-

* Silliman's *Am. Jour. Sci.* July, 1831, vol. xx. pp. 340–343.

† Sturgeon's *Annals of Electricity* etc., March, 1839, vol. iii. p. 430. Sturgeon himself the first to devise a *rotary* electro-magnetic engine, deserves honorable mention for correcting the statement of an American writer, and declining his mistaken award by frankly recognizing Henry's right to priority. (*Annals of Electricity*, April, 1839, vol. iii. p. 554.)

ral to the successful inventor, he carefully scanned the capabilities of this new dynamic agent. Considering the source of the power, he arrived at the conclusion that the deoxidation of metal necessary for the battery, would require the expenditure of at least as much power as its combustion in the battery could refund; and that the coal consumed in such deoxidation could be much more economically employed directly in the work to be done.* As the battery consumption moreover was found to increase more rapidly than the magnetic power produced, he was at once convinced that it could never supersede or compete with steam.† He believed however that the engine had a useful future in many minor applications where economy was not the most important consideration.

When sometime afterward, a friend urged him to secure patents on his inventions,—the "intensity" electro-magnet with its combinations, and the magnetic engine with its automatic pole-changer, earnestly assuring him that either one with proper management would secure an ample fortune to its owner, he firmly resisted every importunity; declaring that he would feel humiliated by any attempt at monopolizing the fruits of science, which he thought belonged to the world. And this aversion to self-aggrandizement by researches undertaken for truth, was carried with him through life.‡

While such disinterestedness cannot fail to excite our admiration, it may perhaps be questioned whether in these cases it did not from a practical point of view, amount to an over-fastidiousness:—whether such legal establishment of ownership, shielding the possessor from the occasional depreciations of the envious,

* These considerations have been more than justified by later comparative investigations. Rankine estimates that the consumption of one pound of zinc will not produce more than one-tenth the energy that one pound of coal will; and that though in the efficient utilization of this energy it is four times superior, its useful work is therefore less than half that of coal; while its cost is from forty to fifty times greater. (*The Steam Engine and other Prime Movers.* By W. J. M. Rankine, London and Glasgow, 1859, part iv. art. 395, p. 541.)

† James P. Joule himself an inventor of an electro-magnetic engine) in a letter dated May 28, 1839, said: "I can scarcely doubt that electro-magnetism will eventually be substituted for steam in propelling machinery." (Sturgeon's *Annals of Electricity*, vol. iv. p. 135.) This was some years before he commenced his investigations on the mechanical equivalent of heat and other motors. He subsequently estimated that the consumption of a grain of zinc though forty times more costly than a grain of coal, produces only about one-eighth of the same mechanical effect.

‡ This trait calls to mind Faraday's avowal made nearly thirty years later, when in a letter to Messrs. Smith and Bentley dated January 3, 1859, (declining the publication of his "Juvenile Lectures") he said: "In fact I have always loved science more than money; and because my occupation is almost entirely personal, I cannot afford to get rich." (Bence Jones' *Life of Faraday*, vol. ii. p. 423.)

and securing by its more tangible remunerations the leisure and the means for more extended researches, would not have been to science more than a compensation for the supposed sacrifice of dignity by the philosopher.*

Nor did this repugnance to patenting arise (as it sometimes does) from any theoretical disapproval of the system. On the contrary, he frequently expressed his strong conviction that a judicious code of patent laws—if faithfully administered—furnishes the most equitable method of recompensing meritorious inventors. The institution was a good one—for others.

The discovery of Magneto-electricity.—From the magnetizing influence of the galvanic current, physicists were almost inevitably led to expect the converse reaction; and this anticipation appears to have been coeval with electro-magnetism. As early as 1820, the illustrious Augustin Fresnel remarked: "It is natural to try whether a magnetic bar will not produce a galvanic current in a helical wire surrounding it;" and he made various experiments to determine a question which was supposed to involve the soundness of Ampère's theory. In November 1820, he announced that though he at first supposed his attempt at the magneto-electric decomposition of water was partially successful, he was finally satisfied that no decisive result was obtained.†

Five years later, Faraday attempted the same experimental inquiry; and among his earliest publications gave an account of his unsuccessful trials. After describing his arrangements he says: "The magnet was then put in various positions and to different extents into the helix, and the needle of the galvanometer noticed: no effect however upon it could be observed. The circuit was made very long, very short, of wires of different metals and different diameters, down to extreme fineness, but the results were always the same. Magnets more and less powerful were used. . . . Hence it appears that however powerful the action of an electric current may be upon a magnet, the latter has no tendency by re-action to diminish or increase the intensity of the former."‡

Nor were American physicists discouraged by the records of repeated failures: and when the great Henry magnet was received at Yale College, Professor C. U. Shepard (Chemical Assistant to Professor Silliman) at once attacked the problem with this new equipment. He remarks: "As its magnetic flow

* Several hundred patents have since been granted in this country for ingenious modifications of—or improvements upon the electro-magnetic telegraph; and probably a hundred for equally ingenious varieties of the electro-magnetic engine; all of which would have been tributary to Henry as an original patentee.

† *Annales de Chimie et de Physique*, 1820, vol. xv. pp. 219–222.

‡ *Quarterly Journal of Science*, July 1825, vol. xix. p. 338.

was so powerful, I had strong hopes of being able to accomplish the decomposition of water by its means. My experiment however proved unsuccessful. . . . I hope however to resume the research hereafter, under more favorable circumstances."*

Henry, unsatisfied with past efforts, determined to pursue the subject in an exhaustive series of experiments; and had reached some momentary indications of the galvanometer, when his experiments were temporarily interrupted. Meanwhile it was announced in May, 1832, that Faraday had secured the long sought prize; though the announcement was brief, and to those eager for particulars, somewhat disappointing. Henry was accordingly induced to publish in the following number of Silliman's Journal (that for July) a sketch of his own trials both before and after the announced discovery. With reference to Faraday's discovery he remarks: "No detail is given of the experiments, and it is somewhat surprising that results so interesting, and which certainly form a new era in the history of electricity and magnetism, should not have been more fully described before this time in some of the English publications. The only mention I have found of them is the following short account from the Annals of Philosophy for April, under the head of Proceedings of the Royal Institution.—'Feb. 17. Mr. Faraday gave an account of the first two parts of his researches in electricity; namely volta-electric induction, and magneto-electric induction. . . . If a wire connected at both extremities with a galvanometer, be coiled in the form of a helix around a magnet, no current of electricity takes place in it. This is an experiment which has been made by various persons hundreds of times, in the hope of evolving electricity from magnetism. But if the magnet be withdrawn from or introduced into such a helix, a current of electricity is produced *while the magnet is in motion*, and is rendered evident by the deflection of the galvanometer. If a single wire be passed by a magnetic pole, a current of electricity is induced through it which can be rendered sensible.'†

"Before having any knowledge of the method given in the above account, I had succeeded in producing electrical effects in the following manner, which differs from that employed by Mr. Faraday, and which appears to me to develop some new and interesting facts. A piece of copper wire about thirty feet long and covered with elastic varnish, was closely coiled around the

* Silliman's *Am. Jour. Sci.* April, 1831, vol. xx. p. 201, foot-note.

† *Philosoph. Mag. and Annals of Phil.* April, 1832, vol. xi. pp. 300, 301. Although Faraday's first communication on galvanic induction, and on magneto-electricity, was read before the Royal Society November 24, 1831, the published Transactions for 1832, containing this memoir did not reach this country till more than a year later: so that the meager abstract of the Royal Institution Proceedings above given, was the only notice of this important discovery,—here accessible for many months.

middle of the soft iron armature of the galvanic magnet described in vol. xix. of the American Journal of Science, and which when excited will readily sustain between six hundred and seven hundred pounds. The wire was wound upon itself so as to occupy only about one inch of the length of the armature, which is seven inches in all. The armature thus furnished with the wire, was placed in its proper position across the ends of the galvanic magnet, and there fastened so that no motion could take place. The two projecting ends of the helix were dipped into two cups of mercury, and these connected with a distant galvanometer by means of two copper wires each about forty feet long. This arrangement being completed, I stationed myself near the galvanometer and directed an assistant at a given word to immerse suddenly in a vessel of dilute acid, the galvanic battery attached to the magnet. At the instant of immersion the north end of the needle was deflected 30° to the west, indicating a current of electricity from the helix surrounding the armature. The effect however, appeared only as a single impulse, for the needle after a few oscillations resumed its former undisturbed position in the magnetic meridian, although the galvanic action of the battery, and consequently the magnetic power still continued. I was however much surprised to see the needle suddenly deflected from a state of rest to about 20° to the east, or in a contrary direction, when the battery was withdrawn from the acid, and again deflected to the west when it was re-immersed. This operation was repeated many times in succession, and uniformly with the same result, the armature the whole time remaining immovably attached to the poles of the magnet, no motion being required to produce the effect, as it appeared to take place only in consequence of the instantaneous development of the magnetic action in one and the sudden cessation of it in the other. . . . From the foregoing facts it appears that a current of electricity is produced for an instant in a helix of copper wire surrounding a piece of soft iron whenever magnetism is induced in the iron; and a current in an opposite direction when the magnetic action ceases; also that an instantaneous current in one or the other direction accompanies every change in the magnetic intensity of the iron.

"Since reading the account before given of Mr. Faraday's method of producing electrical currents, I have attempted to combine the effects of motion and induction." No increase of effect was however observable. On comparing the two methods separately, it was found that while the sudden introduction of the end of a magnetized bar within the helix connected with the galvanometer deflected the needle seven degrees, the sudden magnetization of the bar when within the helix, deflected the needle thirty degrees. A cylindrical iron bar was made to

rotate rapidly on its axis within a stationary helix, by means of a turning lathe, but no result followed.

In the following month (June) by employing a horse-shoe armature (admitting longer coils), Henry succeeded in obtaining vivid sparks from the magnet. "The poles of the magnet were connected by a single rod of iron bent into the form of a horse-shoe, and its extremities filed perfectly flat so as to come in perfect contact with the faces of the poles: around the middle of the arch of this horse-shoe, two strands of copper wire were tightly coiled one over the other. A current from one of these helices deflected the needle one hundred degrees, and when both were used, the needle was deflected with such force as to make a complete circuit. But the most surprising effect was produced when instead of passing the current through the long wires to the galvanometer, the opposite ends of the helices were held nearly in contact with each other, and the magnet suddenly excited: in this case a small but vivid spark was seen to pass between the ends of the wires, and this effect was repeated as often as the state of intensity of the magnet was changed. . . . It appears from the May number of the Annals of Philosophy, that I have been anticipated in this experiment of drawing sparks from the magnet by Mr. James D. Forbes of Edinburgh who obtained a spark on the 30th of March:* my experiments being made during the last two weeks of June. A simple notification of his result is given, without any account of the experiment, which is reserved for a communication to the Royal Society of Edinburgh. My result is therefore entirely independent of his, and was undoubtedly obtained by a different process."

Henry's gratification at the acquisition of the new insight into natural law, quite absorbed all sentiment of personal pride in its independent attainment; and his appreciation and congratulation of Faraday as the first discoverer of magneto-electricity, were hearty and unreserved. He was also particular always to assign to Faraday the first observation of the curious phenomena of momentary galvanic induction; although himself an independent discoverer of the fact.

In the course of these experiments he made a very important original observation on a peculiar case of self-induction, whereby he was enabled to convert a galvanic current of "quantity" into one of "intensity." This entirely new result seemed to contradict all previous experience. He thus concludes his paper: "I may however mention one fact which I have not seen noticed in any work, and which appears to me to belong to the same class of phenomena as those above described. It is this:—when a small battery is moderately excited by diluted acid and its poles

* *Philosoph. Mag. and Annals*, May, 1832, vol. xi. pp. 359, 360.

(which should be terminated by cups of mercury) are connected by a copper wire not more than a foot in length, no spark is perceived when the connection is either formed or broken: but if a wire thirty or forty feet long be used (instead of the short wire), though no spark will be perceptible when the connection is made, yet when it is broken by drawing one end of the wire from its cup of mercury, a vivid spark is produced. . . . The effect appears somewhat increased by coiling the wire into a helix: it seems also to depend in some measure on the length and thickness of the wire. I can account for these phenomena only by supposing the long wire to become charged with electricity which by its reaction on itself projects a spark when the connection is broken."* This is the earliest notice of the curious phenomenon of self-induction in an electric discharge.

Election as Professor at Princeton.—The Trustees of the College of New Jersey at Princeton, were about this time in search of a Professor to fill the chair of Natural Philosophy in that College, made vacant by the resignation of Professor Henry Vethake, who had accepted a Professorship of Natural Philosophy in the recently established University of the City of New York. Professor Henry had already won considerable reputation as a lecturer and teacher, no less than as an experimental physicist. Professor Silliman of Yale College, urging his appointment, wrote: "Henry has no superior among the scientific men of the country." And Professor Renwick of Columbia College (New York) still more emphatically added: "He has no equal."

Professor Henry was unanimously elected by the Trustees;† and he accepted the appointment: although strongly attached to his first Academy, endeared to him by early memories, by six years of successful labors, and by the warm regard of all his associates. May it not be added that his residence at the capital of the State of New York, was further endeared to him by life's romance,—a most congenial and happy marriage contracted in 1830.

ELECTRICAL RESEARCHES AT PRINCETON: FROM 1833 TO 1842.

In November 1832, Henry left the scene of his early scientific triumphs, the Albany Academy, and removed to Princeton with his family. For a year or two he gave his whole attention and

* Silliman's *Am. Jour. Sci.*, July, 1832, vol. xxii. p. 408.

† Dr. Maclean connected with the Faculty of the College of New Jersey at Princeton for fifty years, and for fourteen years its venerable president, in his History of the College (2 vols. 8vo. Philadelphia, 1877,) gives a very interesting account of the appointment and election of Joseph Henry as Professor of Natural Philosophy in 1832, vol. ii. pp. 288–291.

exertions to the duties of exposition and instruction; and during Dr. Torrey's visit to Europe in 1833, at the Doctor's request Professor Henry filled *ad interim* his chair of Chemistry, Mineralogy, and Geology. These occupations left him no leisure for the pursuit of original research. He subsequently gave lectures on Astronomy, and also on Architecture.

In 1834, Henry constructed for the Laboratory of his College an original form of Galvanic Battery; so arranged as to bring into action any desired number of elements, from a single pair to eighty-eight. Each zinc plate 9 inches wide and 12 inches deep was surrounded by a copper case open at top and bottom, and giving thus one and a half square feet of efficient surface. Eleven of these in eleven separate cells, formed a sub-battery; and eight of these were grouped together by means of adjustable conductors, so as to form from the whole a single battery. By means of a crank and windlass shaft in proper connection, any one or more of the eight sub-batteries could be immersed or disengaged, and if desired, a single cell could alone be charged. By another arrangement of adjustable conductors, all the zinc plates could be directly connected together, and all the copper plates together, after the plan of Dr. Hare's "calorimotor" battery; thus giving the "quantity" effect due to a single element of 132 square feet of zinc surface, or of any smaller area desired. As the author remarks concerning its various arrangements, "they have been adopted in most cases after several experiments and much personal labor." A detailed account of this battery was given in a communication read January 16th 1835, before the American Philosophical Society (of which he had recently been elected a member), and was published in its Transactions.*

Meanwhile he had been engaged in his brief intervals of relaxation from his exacting professional cares during the past year, in repeating and extending his interesting observations (commenced at Albany in 1832), on the remarkable intensifying influence of a long conductor, and especially of a spiral one, when interposed in a galvanic circuit of a single pair, or a battery of low "intensity." A verbal communication on this curious form of "induction," was made to the Society on the same occasion as the description of his battery, and was illustrated by experiments exhibited before the Society.

Faraday in his "eighth series of Researches" (read before the Royal Society June 5th 1834), pointed out very fully the differing actions of a single galvanic element giving a "quantity" current, and of a series of elements giving an "intensity" current:† thus entirely confirming the results obtained by Henry more than three years previously.

* *Trans. Am. Philos. Soc.* vol. v. new series, art. iv. pp. 217-222.

† *Phil. Trans. Royal Soc.* June 5, 1834, vol. cxxiv. art. 990-994, pp. 445, 446. *Experimental Researches in Electricity*, vol. i. pp. 301, 302.

In the Philosophical Magazine for November, 1834, appeared a paper by Faraday, "On a peculiar condition of electric and magneto-electric Induction:" in which he notices as a remarkable fact, that while a short circuit wire from a single galvanic element, gives little or no visible spark, a long conductor gives a very sensible spark. "It is however very interesting thus to observe an original current of electricity having a very low intensity producing ultimately a counter current having an intensity probably a hundred fold greater than its own, and the experiment constitutes one of the very few modes we have at command of converting quantity into intensity as respects electricity in currents." And he remarks: "If the connecting wire be much lengthened, then the spark is much increased."*

In this interesting research, Faraday appears to have entirely overlooked Henry's earlier labors in the same field;—as contrary to his usual custom, he makes no allusion to the same results having been obtained, and published in Silliman's Journal two years and a half before.† These observations were made by Faraday the subject of his "ninth series of Researches," in a communication "On the influence by induction of an electric current on itself:" read before the Royal Society January 29th 1835. In this paper he repeats, that with a single galvanic pair, a short wire will not give a shock; while the wire surrounding an electro-magnet will give a shock at each breaking of the circuit. He found a similar result with the wire helix alone,—without its magnetic core "The power of producing these phenomena exists therefore in the simple helix, as well as in the electro-magnet, although by no means in the same high degree." With continuous straight wire of the same length, he obtained a similar effect,—"yet not so bright as that from the helix" When a short wire is used, "all these effects disappear;" although there is undoubtedly a greater "quantity" of electric current in the shorter wire; thus giving "the strange result of a diminished spark and shock from the strong current, and increased effects from the weak one."‡

While Henry derived only satisfaction from these extended verifications of his own observations, by one whom he had accustomed himself to look up to with admiration and regard, Dr. A. Dallas Bache, his attached friend, then Professor of Natural Philosophy in the University of Pennsylvania,—more jealous than himself of his scientific fame, strongly urged and insisted that he should immediately publish an account of his later researches. Henry accordingly sent to the American Philosophical

* *Philosoph. Mag.* Nov. 1834, vol. v. pp. 351, 352.

† Sill. *Am. Jour. Sci.* July. 1832. vol. xxii. p. 408. above quoted.

‡ *Phil. Trans. Royal Soc.* Jan. 29, 1835, vol. cxxv. art. 1061–1067. and 1073, pp. 43-45. *Experimental Researches in Electricity*, vol. i. pp. 325–328. This memoir did not reach this country of course till a year later.

Society a memoir (comprising the details of his recent verbal communication) "On the Influence of a Spiral Conductor in increasing the Intensity of Electricity from a galvanic arrangement of a Single pair, etc.," which was read before the Society, February 6th, 1835.

After citing his former paper of July, 1832, the writer remarks that he had been able during the past year to extend his experiments on the curious phenomenon. "These though not so complete as I could wish, are now presented to the Society with the belief that they will be interesting at this time on account of the recent publication of Mr. Faraday on the same subject." He then relates that employing a single pair of his battery (comprising one and a half square feet of zinc surface), he found as in his earlier experiment in 1832, that the poles being connected by a piece of copper bell-wire five inches long, no spark was given on making or breaking contact. Fifteen feet of interposed wire gave a very feeble spark; and with successive additions of fifteen feet, the effect increased until with 120 feet the maximum spark appeared to be reached, and beyond this there was no perceptible increase; while with double this length (or 240 feet) there seemed to be a diminution of intensity. From various trials the inference was drawn that the length required for maximum effect varied with the size of the galvanic element. Thicker wires of the same length produced greater effect, depending in some degree on the size of the battery. A wire of forty feet when coiled into a cylindrical helix "gave a more intense spark than the same wire uncoiled." A ribbon of sheet copper about an inch wide and twenty-eight feet long, being covered with silk and coiled into a flat spiral—like a watch spring—(after the plan of Dr. Richie) gave a vivid spark with a loud snap. When uncoiled, it produced a much feebler spark. With the insulated copper ribbon folded in its middle, and the double thickness coiled into a flat spiral, there was no spark whatever, although the same ribbon unrolled gave a feeble spark: thus showing that the induction of the current upon itself was neutralized by flowing equally in opposite directions in the double spiral. With a larger copper ribbon one inch and a half wide, and 96 feet long, spirally coiled (weighing 15 pounds) the snap of the spark could be heard in an adjoining room with the door closed. Want of material prevented the result being pushed further, so as to ascertain the range of maximum effect with this form of conductor. With increased battery surface, the effect was also increased; so that with eight elements of his battery arranged as a single pair (of 12 square feet) the spark on breaking contact "resembled the discharge of a small Leyden jar highly charged." With the flat spiral, no increase of effect was observable on the introduction of a soft iron core into the axis of the spiral, forming a magnet. With a helical or cylindrical coil about nine inches long, enclosing

an iron core, "the spark appeared a little more intense than without the iron." The inference is also drawn "from these experiments, that some of the effects heretofore attributed to magneto-electric action are chiefly due to the reaction on each other of the several spirals of the coil which surround the magnet."

In these researches it was found that when the two plates of a single pair were placed even fourteen inches apart in an open trough of diluted acid, "although the electrical intensity in this case must have been very low, yet there was but little reduction in the apparent intensity of the spark." It was also shown that "the spiral conductor produces however, little or no increase of effect when introduced into a galvanic circuit of considerable intensity." When for example an "intensity" battery of two Cruickshank's troughs, each containing 56 elements was employed with the larger copper spiral, "no greater effect was perceived than with a short thick wire:" in either case, only a feeble spark being given.* An abstract of the results thus announced (and which were obtained by Henry during the summer of 1834,) was communicated by Dr. A. D. Bache, as a Secretary of the American Philosophical Society, to the Franklin Journal, in order to give these interesting facts an earlier currency.† The date of original discovery was however so well established, that this friendly effort was scarcely necessary.‡

Combined Circuits.—In 1835, or early in 1836, wires had been extended across the front campus of the College grounds at Princeton from the upper story of the library building to the Philosophical Hall on the opposite side, through which signals were occasionally sent, distinguished by the number of taps of the electro-magnetic bell, first exhibited five years previously in the hall of the Albany Academy. It has already been noticed, that contrary to all the antecedent expectations of physicists, Henry had established the fact that the most powerful form of magnet (designated by him the "quantity" magnet) is not the form best adapted to distant action through an extended circuit. The ingenious idea occurred to him that notwithstanding this fundamental fact, it would be quite easy to combine the two systems so as to enable an operator to produce the most energetic mechanical effects, at almost any required distance.

* *Trans. Am. Phil. Soc.* vol. v. n. s. art. x. pp. 223-231.

† *Journal of the Franklin Institute*, March, 1835, vol. xv. pp. 169, 170.

‡ M. Becquerel in his elaborate Treatise on Electricity, in the chapter on "The influence of an electric current on itself by induction," says with regard to the increase of tension in a feeble current when passing through a long spiral conductor, "The effects observed in these circumstances appear to have been noticed for the first time by Professor Henry." (*Traité experimental de l'Électricité et du Magnétisme*, 8vo. 7 vols. Paris, 1824-1840, vol. v. art. 1261, p. 231.)

It is simply necessary to employ with the distant "intensity" magnet an oscillating armature with a suitable prolongation so arranged as to open and close the short circuit of an adjoining "quantity" magnet of any practicable power:—a work which indeed could be accomplished by the mere swing of the most delicate galvanometer needle. Professor Henry had constructed for his own laboratory a large electro-magnet designed to surpass the celebrated magnet made for Yale College; and with it he was enabled to exhibit to his class, by employing a small portion of his "quantity" battery, an easy lifting power of more than three thousand pounds.* Such was the mechanical agency he called into action through his telegraphic circuit, by simply lifting its galvanic wire from a mercury thimble, or by again dipping it into the same. Although this special combination has not found any important application, its principle underlies all the various forms and uses of the "relay" magnet and local battery since employed.

Visit to Europe.—In order to give Professor Henry a much-needed rest from his diligent services and close application of the past four years, the Trustees of his College liberally allowed him a year's absence with full salary: thus affording him for the first time a long coveted opportunity of visiting Europe.

In February of 1837, in company with his valued and faithful friend, Professor Bache, he arrived in England; where the two American physicists formed ready and lasting intimacies with some of the most distinguished worthies of Great Britain. Everywhere received with courteous and cordial consideration, they both ever carried with them agreeable memories of their holiday sojourn abroad.

In London, many pleasant interviews with Faraday, formed a memorable circumstance. Wheatstone, then Professor of Experimental Philosophy in King's College, was engaged in developing his system of needle telegraph: and Henry had the satisfaction of finding that his own early investigations were recognized and appreciated, and their results successfully adopted. Wheatstone unfolded freely to his visitors his numerous projects; and particularly his arrangement of supplementary local circuit from an additional battery, for sounding an electro-magnetic signal, by being brought into action by a movement from the main line circuit.† Henry had then the pleasure of detailing to him his own similar combination of two electro-

* It is said that this magnet has been made to sustain 3500 pounds.

† This was early in April, 1837. (*Smithsonian Report* for 1857, p. 111.) Two months later, or June 12th, 1837, Wheatstone in conjunction with W. F. Cooke had secured a patent on his system of telegraph, including the combination of circuits.

magnetic circuits, experimentally tried more than a year previously.

Nearly a year was employed in foreign travel, most pleasantly and beneficially both for mind and body: the greater portion of the time however being spent in London, in Paris, (where Henry formed the acquaintance of Arago, Becquerel, De la Rive, Biot, Gay-Lussac, and other celebrities,) and in Edinburgh, where he also found a galaxy of eminent and congenial minds.

In September of the same year (1837) he attended the meeting of the British Association at Liverpool; where being invited to speak, he made a brief communication on some electrical researches in regard to the phenomenon known as the "lateral discharge:"—a study to which he had been led by some remarks of Dr. Roget on the subject. "The result of the analysis was in accordance with an opinion of Biot—that the lateral discharge is due only to the escape of the small quantity of redundant electricity which always exists on one side or the other of a jar, and not the whole discharge." Hence we could increase or diminish the lateral action by any means which affect the quantity of free electricity:—as by "an increase of the thickness of the glass, or by substituting for the small knob of the jar, a large ball. But the arrangement which produces the greatest effect is that of a long fine copper wire insulated,—parallel to the horizon, and terminated at each end by a small ball. When sparks are thrown on this from a globe of about a foot in diameter, the wire at each discharge becomes beautifully luminous from one end to the other, even if it be a hundred feet long: rays are given off on all sides perpendicular to the axis of the wire:"—forming a continuous electrical brush. It was also stated "that the same quantity of electricity could be made to remain on the wire, if gradually communicated [by a point]; but when thrown on in the form of a spark, it is dissipated as before described:"—as though possessing a kind of momentum. When two or more wires are arranged in parallel lines (in electrical connection), only the outer sides of the exposed wires become luminous: and "when the wire is formed into a flat spiral, the outer spiral alone exhibits the lateral discharge, but the light in this case is very brilliant: the inner spirals appear to increase the effect by induction." In like manner when a ball was attached to the middle of a vertical lightning-rod having a good earth-connection, "when sparks of about an inch and a half were thrown on the ball, corresponding lateral sparks could be drawn not only from the parts of the rod between the ground and the ball, but from the part above, even to the top of the rod." *

At the same meeting, before the section on Mechanics and

* *Report of Brit. Association*, for 1837, pp. 22–24, of Abstracts.

Engineering, Henry gave by request an account of the great extension of the Railway and Canal systems in the United States: which was listened to with great attention and interest. He also referred to the inland or river navigation in our country, describing the improvements introduced into our large river steam-boats, especially on the Hudson river in New York State; where the usual speed was fifteen miles per hour or more.*

In November, 1837, Henry returned from his foreign tour greatly invigorated,—bringing with him some new apparatus: and with increased zest he re-embarked upon the duties of his professorship. Continuing his studies of electrical action, he presented verbally to the American Philosophical Society, February 16th 1838, a notice of further observations on the "lateral discharge" of electricity while passing along a wire. going to show that even with good earth connection, free electricity is not conducted silently to the ground.†

In May, 1838, he announced to the Society the production of currents by induction from ordinary or mechanical electricity, analogous to that first obtained by Faraday from galvanism in 1831: and the further curious fact that on the discharge from a Leyden jar through a good conductor, a secondary shock from a perfectly insulated near conductor could be obtained more intense than the primary shock directly from the jar.‡

These investigations having in view the discovery of "inductive actions in common electricity analogous to those found in galvanism" (commenced in the Spring of 1836), led to renewed examination of the secondary *galvanic* current, which since November 24th, 1831, (or for seven years,) had received no special attention. Henry's very interesting series of experiments were detailed in a somewhat elaborate memoir read before the American Philosophical Society, November 2nd, 1838. Employing five different sized annular spools of fine wire (about one-fiftieth of an inch thick) varying from one-fifth of a mile to nearly a mile in length (which might be called "intensity" helices); and six flat spiral coils of copper ribbon varying from three-quarters of an inch to one inch and a half in width, and from 60 to 93 feet in length (which might be called "quantity" coils), he was able to combine them in various ways both in connection and in parallelism. A cylindrical battery of one and three quarters square

* Same *Report*, Abstracts, p. 135. It was on this occasion that Dr. Lardner, generalizing probably from his observations on the Thames, ventured (not very courteously) to doubt whether any such speed as fifteen miles per hour on water, could ordinarily be effected. (Sill. *Am. Jour. Sci.* Jan. 1838, vol. xxxii. p. 296.) The same authority affirmed the futility of attempting oceanic steam navigation.

† *Proceedings Am. Phil. Soc.* Feb. 16, 1838, vol. i. p. 6.

‡ *Proceedings Am. Phil. Soc.* May 4, 1838, vol. i. p. 14.

feet of zinc surface was principally used; and the galvanic circuit was interrupted by drawing one end of the copper ribbon or wire over a rasp in good metallic contact with the other pole of the battery.

From the energetic action of the flat ribbon coil in producing the induction of a current on itself, it was inferred that the secondary current would also be best induced by it. With the single larger ribbon coil in connection with the battery, and another ribbon coil placed over it resting on an interposed glass plate, at every interruption of the primary circuit, an induction spark was obtained at the rubbed ends of the second coil; though the shock was feeble. With a double wire spool (one within the other) of 2650 yards, placed above the primary coil (having about the same weight as the copper ribbon) the magnetizing effects disappeared, the sparks were much smaller, "but the shock was almost too intense to be received with impunity." The secondary current in this case was one of small "quantity" but of great "intensity." With a single break of circuit in the primary, it was passed through a circle of 56 students of his senior class, with the effect of a moderate charge from a Leyden jar. From various experiments, the limit of efficient length for a given galvanic power was ascertained; beyond which the induced current was diminished. Employing a Cruickshank battery of 60 small elements (4 inches square) he found with the ribbon coil that the induced currents were exceedingly feeble, but with the long wire helix as the primary circuit that strong indications were produced. By the alternations of the ribbon and wire coils, the fact was established "that an intensity current can induce one of quantity, and by the preceding experiments the converse has also been shown that a quantity current can induce one of intensity;" a result which has had an important bearing on the subsequent development of the electro-magnetic "Induction-Coil." With a long ribbon coil receiving the galvanic current from 35 feet of zinc surface, sensible induction shocks could be felt from a large annular coil of four feet diameter (containing five miles of wire) when placed in parallelism at a distance of four feet from the primary coil: while at the distance of one foot the shock became too severe to be taken. With this arrangement an induction shock was given from one apartment to another, through the intervening partition.

Successive orders of Induction.—When it is considered that the primary current in such cases has a considerable duration, while the secondary current is but momentary, being developed only at the instant of change in the primary, it could certainly not have been expected that this single instantaneous electrical impulse of reaction would be capable of acting as a primary current, and of similarly inducing an action on a third independ-

ent circuit: and during the seven years in which galvanic induction had been known, no physicist ever thought of making the trial. Theoretically it might perhaps have been inferred, if such tertiary induction had any existence, as it would be coincident not with the instantaneous secondary induction, but with the initiation and termination of such momentary current, and hence in opposite signs—separated by an inappreciable interval of time, that the whole phenomenon would probably be entirely masked by a practical neutralization. The experiments of Henry fully established, however, the new and remarkable result of a very appreciable tertiary current. By connecting the secondary coil with another at some distance from the primary so as not to be influenced by it directly, but forming with the secondary a single closed circuit, not only was the distant coil capable of producing in an insulated wire helix placed over it, a distinct current of induction at the interruption of the primary, but sensible shocks were obtained from it. The experiment was pushed still further; and inductive currents of a fourth degree were obtained. "By a similar but more extended arrangement, shocks were received from currents of a fourth, and a fifth order: and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained. . . . It was found that with the small battery a shock could be given from the current of the third order to twenty-five persons joining hands; also shocks perceptible in the arms were obtained from a current of the fifth order." As Henry simply remarks: "The induction of currents of different orders, of sufficient intensity to give shocks, could scarcely have been anticipated from our previous knowledge of the subject." By means of the small magnetizing helix introduced into each circuit, the direction of these successive currents was found to be alternating or reversed to each other.

The concluding section of this important memoir is occupied with an account of "The production of induced currents of the different orders from ordinary electricity." An open glass cylinder about six inches in diameter was provided with two long narrow strips of tin foil pasted around it in corresponding helical courses, the one on the outside and the other on the inside, directly opposite to each other. The inner coiled strip had its extremities connected with insulated wires which formed a circuit outside the cylinder, and included a small magnetizing helix. The outer tin foil strip was also connected with wires so that an electrical discharge from a half-gallon Leyden jar could be passed through it. The magnetization of a small needle indicated an induced current through the inner tin-foil ribbon corresponding in direction with the outer current from the jar.* By means of

* About a year later, the distinguished German electrician Peter Riess, apparently unaware of Henry's researches, discovered the secondary cur-

a second glass cylinder similarly provided with helical tin-foil ribbons in suitable connections a tertiary current of induction was obtained, analogous to that derived from galvanism. "Also by the addition in the same way of a third cylinder, a current of the fourth order was developed."

Similar as these successive inductions from an electrical discharge were to those previously observed in the case of the galvanic current, they presented one puzzling difference in the directions of the currents of the different orders. "These in the experiments with the glass cylinders, instead of exhibiting the alternations of the galvanic currents, were all in the same direction as the discharge from the jar, or in other words they were all *plus*." On substituting for the tinned glass cylinders, well insulated copper coils, "alternations were found the same as in the case of galvanism." The only difference apparently between the two arrangements, was that the tin-foil ribbons were separated only by the thin glass of the cylinders, while the copper spiral coils were placed an inch and a half apart. By varied experiments, the direction of the induced currents was found to depend notably on the distance between the conductors;—the induction ceasing at a certain distance, (according to the amount of the charge and the characters of the conductors,) and the direction of the induced current beyond this critical distance being contrary to that of the primary current.* "With a battery of eight half-gallon jars, and parallel wires about ten feet long, the change in the direction did not take place at a less distance than from twelve to fifteen inches, and with a still larger battery and longer conductors, no change was found although the induction was produced at the distance of several feet." With Dr. Hare's battery of 32 gallon jars, and a copper wire about one-tenth of an inch thick and 80 feet long stretched across the lecture-room and back on either side toward the battery, a second wire stretched

rent induced from mechanical electricity, by a very similar experiment. (Poggendorff's *Annalen der Physik und Chemie*, 1839, No. 5, vol. xlvii. pp. 55–76.)

* The variation in the direction of polarization (without reference to induction currents) appears to have been first noticed by F. Savary, some dozen years before. In an important memoir communicated to the Paris Academy of Sciences July 31, 1826, M. Savary announced that "The direction of the magnetic polarity of small needles exposed to an electric current directed along a wire stretched longitudinally, varies with the distance of the wire:"—the action being found to be periodical with the distance. M. Savary observed three periods, and also the fact that the distances of maximum effect and of the nodal zeros "vary with the length and diameter of the wire, and with the intensity of the discharge." He also found that "when a helix is used for magnetizing, the distance at which the needle placed within it is from the conducting wire, is indifferent; but the direction and the degree of magnetization depends on the intensity of the discharge, and on the ratio between the length and size of the wire." (Brewster's *Edinburgh Jour. Sci.* Oct. 1826, vol. v. p. 369.)

parallel with the former for about 35 feet and extended to form an independent circuit (its ends being connected with a small magnetizing helix,) was tested at varying distances beginning with a few inches until they were twelve feet apart: at which distance of the parallel wire, its induction though enfeebled, still indicated by its magnetizing power, a direction corresponding with the primary current. The form of the room did not permit a convenient separation of the two circuits to a greater distance.*

The eminent French electrician Becquerel, in a chapter on Induction in his large work, remarks: "Quite recently M. Henry, Professor of Natural Philosophy in New Jersey, has extended the domain of this branch of physics: the results obtained by him are of such importance, particularly in regard to the intensity of the effects produced, that it is proper to expound them here with some detail." Twenty pages are then devoted to these researches.†

A memoir was read before the Society, June 19th, 1840, giving an account of observations on the two forms of induction occurring on the making and on the breaking of the primary galvanic circuit, the two differing in character as well as in direction. In these experiments he employed a Daniell's constant battery of 30 elements; the battery being "sometimes used as a single series with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity as the occasion might require." As the initial induction had always been found so feeble as to be scarcely perceptible, (although in quantity sufficient to affect the ordinary galvanometer as much as the terminal induction,) most of the results previously obtained (such as the detection of successive orders of currents) were derived from the strong inductions at the moment of breaking the circuit. It became therefore important to endeavor to intensify the initial induction for its more especial examination: and this it was found could be effected in two ways,—by increasing the "intensity" of the battery, and by diminishing within certain limits the length of the primary coil.

"With the current from one element, the shock at breaking the circuit was quite severe, but at making the same it was very feeble, and could be perceived in the fingers only or through the

* *Trans. Am. Phil. Soc.* vol. vi. new series, art. ix. pp. 303-337. In the Proceedings of the Society for November 2d, 1838, when this memoir was read, it is recorded "Professor Henry made a verbal communication during the course of which he illustrated experimentally the phenomena developed in his paper." (*Proceed. Am. Phil. Soc.* Nov. 2, 1838, vol. i. p. 54.)

† *Traité experimental de l'Électricité et du Magnétisme*, vol. v. pp. 87-107.

tongue. With two elements in the circuit the shock at the beginning was slightly increased: with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to ten, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly greater than the latter, the difference continually increasing until all the thirty elements were introduced into the circuit. . . . Experiments were next made to determine the influence of a variation in the length of the coil, the intensity of the battery remaining the same." For this purpose the battery consisting of a single element "was employed; and the length of the copper ribbon coil was successively reduced from 60 feet, by measures of 15 feet. With 45 feet, the initial induction was stronger than with 60 feet: with the next shorter length it was more perceptible, and increased in intensity with each diminution of the coil, until a length of about fifteen feet appeared to give a maximum result." At the same time it was found that "the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil. . . . By the foregoing results we are evidently furnished with two methods of increasing at pleasure the intensity of the induction at the beginning of a battery current, the one consisting in increasing the intensity of the source of the electricity, and the other in diminishing the resistance to conduction of the circuit while its intensity remains the same."

Having thus succeeded in exalting the initial induction, Henry proceeded in his investigation. Distinct currents of the third, fourth, and fifth orders were readily obtained from it; and as was anticipated, with their signs (or directions) the reverse of the corresponding orders derived from the terminal induction. In other respects "the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current."

In the course of these investigations the idea having occurred to him "that the intense shocks given by the electrical fish may possibly be from a secondary current," as it appeared to him that "this is the only way in which we can conceive of such intense electricity being produced in organs imperfectly insulated and immersed in a conducting medium," he endeavored to simulate the effect by arranging a secondary wire coil furnished with terminal handles, over a primary copper ribbon coil, the two being insulated as usual. "By immersing the apparatus in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged

into the water parallel to a line joining the two poles, a shock is felt through the arms."

The former experiment of obtaining an induction shock from one room to another through a partition, was repeated on a still larger scale. All the coils of copper ribbon having been united in a single continuous conductor of about 400 feet in length, "this was rolled into a ring of five and a half feet in diameter, and suspended vertically against the inside of the large folding doors which separate the laboratory from the lecture-room. On the other side of the doors in the lecture-room and directly opposite the coil was placed a helix formed of upwards of a mile of copper wire, one-sixteenth of an inch in thickness, and wound into a hoop of four feet in diameter. With this arrangement, and a battery of 147 square feet of zinc surface divided into eight elements, shocks were perceptible in the tongue when the two conductors were separated to the distance of nearly seven feet. At the distance of between three and four feet, the shocks were quite severe. The exhibition was rendered more interesting by causing the induction to take place through a number of persons standing in a row between the two conductors."

The second section of the memoir is mainly occupied with details of experiments on the screening effect of conducting plates (of non-magnetic metals) when interposed between the primary and secondary coils: showing remarkable contrasts in the "quantity" and "intensity" classes of galvanic effects. When the annular spool or helix (of nearly one mile of copper wire) was employed with the large spiral coil of copper ribbon, "the coil being connected with a battery of ten elements, the shocks both at making and breaking the circuit were very severe; and these as usual were almost entirely neutralized by the interposition of the zinc plate. But when the galvanometer was introduced into the circuit instead of the body, its indications were the same whether the plate was interposed or not: or in other words the galvanometer indicated no screening, while under the same circumstances the shocks were neutralized. A similar effect was observed when the galvanometer and the magnetizing helix were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the helix (in reference to tempered steel) while the deflections of the galvanometer were unaffected." The induction currents of the third, fourth, and fifth orders, were found to be of considerable "intensity;"—magnetizing steel needles, giving shocks, not being interrupted by a drop of water placed in the circuit between the ends of the severed wire,—and yet being screened or neutralized by a metallic plate interposed between the coils.*

* *Trans. Am. Phil. Soc.* June 1840, vol. viii. n. s. art. i. pp. 1–18.

A continuation of the memoir was read before the Philosophical Society November 20th, 1840, discussing further the theoretical differences between an initial or an increasing galvanic current, and a decreasing or a terminal current, in producing the phenomena of induction. On the same occasion Henry described "an apparatus for producing a reciprocating motion by the repulsion in the consecutive parts of a conductor through which a galvanic current is passing." About ten years before, he had devised the first electro-magnetic engine (operating by intermittent magnetic attractions and repulsions); and now he had contrived the first galvanic engine, operating by the analogous intermittent attractions and repulsions of the electric current.*

In June 1842, he presented a communication to the Society recounting an investigation of some anomalies in ordinary electrical induction. While with the larger needles ("No. 3 and No. 4") subjected to the magnetizing helix, the polarity was always conformable to the direction of the discharge, he found that when very fine needles were employed, an increase in the force of the electricity produced changes of polarity. About a thousand needles were magnetized in the testing helices in these researches. This puzzling phenomenon was finally cleared up by the important discovery that an electrical equilibrium was not instantaneously effected by the spark, but that it was attained only after several oscillations of the flow. "The discharge, whatever may be its nature is not correctly represented by the single transfer from one side of the jar to the other: the phenomena require us to admit the existence of a principal discharge in one direction, and then several reflex actions backward and forward, each more feeble than the preceding, until the equilibrium is obtained."† In every case therefore of the electrostatic discharge, the testing needles were really subjected to an oscillating alternation of currents, and consequently to successive partial de-magnetizations and re-magnetizations. The complications produced by this residual action, satisfactorily explained for the first time, the discordant results obtained by different investigators. This singular reflux of current was ingeniously applied by Henry to explain the apparent change of inductive current with differing distances. Should the primitive discharge wave be in excess of the magnetic

* *Proceedings Am. Phil. Soc.* Nov. 20, 1840, vol. i. p. 301.

† *Proceedings Am. Phil. Soc.* June 17, 1842, vol. ii. pp. 193.—Helmholtz some five years later (in 1847), but quite independently, suggested "a backward and forward motion between the coatings" when the Leyden jar is discharged. And still five years later (in 1852) Sir William Thomson made the same independent conjecture. To F. Savary however is due the credit of having first thrown out the hypothesis of electrical oscillations, as early as in 1827.

capacity of the needle at a given position, the return wave might be just sufficient to completely reverse its polarity, and the diminished succeeding wave insufficient to restore it to its former condition; while at a greater distance, the primitive wave might be so far reduced as to just magnetize the needle fully, and the second wave, being still more enfeebled, would only partially demagnetize it, leaving still a portion of the original polarity; and so for the following diminished oscillations.

In the course of these extended researches the presence of inductive action was traced to most surprising and unimagined distances. "A single spark from the prime conductor of the machine, of about an inch long, thrown on the end of a circuit of wire in an upper room, produced an induction sufficiently powerful to magnetize needles in a parallel circuit of wire placed in the cellar beneath, at a perpendicular distance of thirty feet, with two floors and ceilings—each fourteen inches thick intervening."

"The last part of the series of experiments relates to induced currents from atmospheric electricity. By a very simple arrangement, needles are strongly magnetized in the author's study, even when the flash is at the distance of seven or eight miles, and when the thunder is scarcely audible. On this principle he proposes a simple self registering electrometer, connected with an elevated exploring rod." For obtaining the results above alluded to, a thick wire was soldered to the edge of the tin roof of his dwelling and passed into his study through a hole in the window frame; while a similar wire passing out to the ground, terminated in connection with a metal plate in a deep well close by. Between the wire ends within his study, various apparatus, including magnetizing helices of different sizes and characters could be attached, so as to be within the line of conduction from the roof to the ground. The inductions from atmospheric discharges were found to have the oscillatory character observed with the Leyden jar; and by interposing several magnetizing helices with few and with many convolutions, he was able to get from a needle in the former the polarity due to the direct current, and in the latter, that due to the return current; thus catching the lightning (as it were) upon the rebound.

In examining the "lateral discharge" from a lightning-rod in good connection with the earth, he had often observed that while a spark could be obtained sufficiently strong to be distinctly felt, it scarcely affected in the slightest degree a delicate gold-leaf electroscope. How explain so incongruous a phenomenon? Henry detected the very simple solution, by a reference to the self-induction of the rod,—a negative wave passing followed immediately by a positive wave so rapidly as to completely neutralize the effect upon the electroscope before the inertia of the gold leaf could be overcome, while actually producing a double spark (sensibly co-incident) to and from the recipient.

A few months later, "he had succeeded in magnetizing needles by the secondary current, in a wire more than two hundred and twenty feet distant from the wire through which the primary current was passing, excited by a single spark from an electrical machine."* In this case the primary wire was his telegraph line stretched seven years before across the campus of the College grounds in front of Nassau Hall: the secondary or induction wire being suspended in a parallel direction across the grounds at the rear of Nassau Hall, with its ends terminating in buried metallic plates:—the large building intervening between the two wires.

This brilliant series of contributions to our knowledge of a most recondite and mysterious agent, placed Henry, by the concurrent judgment of all competent physicists, in the very front rank of original investigators. His persevering researches in the electrical paradoxes of induction, perhaps more than any similar ones, tended to strengthen the hypothesis of an ætherial dynamic agency; although he himself had for a long time been inclined to favor the material hypothesis.†

INVESTIGATIONS IN GENERAL PHYSICS: FROM 1830 TO 1846.

In order to give a proper connection to the experimental inquiries undertaken by Henry in various fields, it is necessary to pause here, and to recur to some of his earlier scientific labors.

Meteorology.—From an early date Henry took a deep interest in the study of meteorology: not only on account of its practical importance, but from its relation to cosmical physics, and because from the very complexity and irregularity of its conditions, it challenged further investigation and stood in need of larger generalizations. His early association with Dr. T. Romeyn Beck in the first development of the system of meteorological observations established in the State of New York, has already been referred to in the sketch of his "Early Career." This active and zealous co-operation continued from 1827 to 1832; or as long as he resided in Albany.

In September of 1830, he commenced a series of observations for Professor Renwick of Columbia College, to determine the magnetic intensity at Albany. With the assistance of his brother-in-law, Professor Stephen Alexander, these observations

* *Proceedings Am. Phil. Soc.* Oct 21, 1842, vol. ii. p. 229.

† In a paper "On the Theory of the so-called Imponderables" published some years later, in referring to the phenomena of electrical oscillation in discharge, and of the series of inductions taking place and "extending to a surprising distance on all sides," he remarks: "As these are the results of currents in alternate directions, they must produce in surrounding space a series of *plus* and *minus* motions, analogous to—if not identical with undulations." (*Proceed. Amer. Association*, Albany, Aug. 1851, p. 89.)

were continued daily for two months.* In April, 1831, a second series of observations was commenced; in the course of which his attention was attracted by a great disturbance of the needle during the time of a conspicuous "aurora" on the 19th of April, 1831. At noon of the 19th the oscillations were found to be perfectly accordant with previous ones, but at 6 o'clock P. M. a remarkable increase of magnetic intensity was indicated. At 10 o'clock of the same evening, during the most active manifestation of the aurora, the oscillations of the needle were again examined. "Instead of still indicating as at 6 o'clock an uncommonly high degree of magnetic intensity, it now showed an intensity considerably lower than usual." Thus, designating the normal intensity at the place as unity, at 6 o'clock it had increased to 1.024, and at 10 o'clock had subsided to 0.993, which according to Hansteen's observations is the usual relation of magnetic disturbance by an aurora.† An account of these results was communicated by Henry to the Albany Institute, January 26, 1832; and was also published in the Report of the Regents of the New York University. A little more than a month later (to wit on March 6, 1832,) he had been able to collate the various published accounts of this aurora; and he learned "the fact of a disturbance of terrestrial magnetism being observed by Mr. Christie in England on the same evening, and at nearly the same time the disturbance was witnessed in Albany, and that too in connection with the appearance of an aurora." This circumstance led him to make a careful comparison of the notices of auroral displays given in the meteorological reports in the Annals of Philosophy for 1830 and 1831, with those of the Reports of the New York Regents for the same period. "By inspecting these two publications it was seen that from April, 1830, to April, 1831,

* The needles employed in these observations were a couple received by Professor Renwick from Capt. Sabine,—one of which had belonged to Prof. Hansteen of Norway. "They were suspended according to the method of Hansteen in a small mahogany box, by a single fibre of raw silk. The box was furnished with a glass cover, and had a graduated arc of ivory on the bottom to mark the amplitude of the vibrations. In using this apparatus, the time of three hundred vibrations was noted by a quarter-second watch, well regulated to mean time; a register being made at the end of every tenth vibration, and a mean deduced from the whole, taken as the true time of the three hundred vibrations. Experiments carefully made with this apparatus were found susceptible of considerable accuracy:" the individual observations not differing from the mean number, ordinarily more than one-thousandth. (Silliman's *Am. Jour. Sci.* April, 1832, vol. xxii. p. 145.)

† Prof. Hansteen has remarked that "A short time before the aurora borealis appears, the intensity of the magnetism of the earth is apt to rise to an uncommon height; but so soon as the aurora borealis begins, in proportion as its force increases, the intensity of the magnetism of the earth decreases, recovering its former strength by degrees, often not till the end of twenty-four hours." (*Edinburgh Philosoph. Jour.* Jan. 1825, vol. xii. p. 91.)

inclusive, the aurora was remarkably frequent and brilliant both in Europe and in this country; and that most of the auroras described in the Annals for this time, particularly the brilliant ones, were seen on the same evening in England and in the State of New York." From which he argues that "these simultaneous appearances of the meteor in Europe and America would therefore seem to warrant the conclusion that the aurora borealis cannot be classed among the ordinary local meteorological phenomena, but that it must be referred to some cause connected with the general physical principles of the globe; and that the more energetic action of this cause (whatever it may be) affects simultaneously a greater portion of the northern hemisphere." *

In attempting to classify and digest the meteorological data within his reach, Henry became strongly impressed with the necessity of much more extensive, continuous, and systematic observations than any as yet undertaken: and he omitted no opportunities of directing influence upon the minds of our national legislators, to impress them with the great need—as well as the practical policy of prosecuting the subject by governmental resources. No one at that day seemed so fully awake both to the importance and to the methods of prosecuting such inquiry: and no one more effectually advanced both by direct and by indirect exertions the wide-spread interest in this study, than he.

In 1839, while at Princeton, he induced the American Philosophical Society officially to memorialize the National Government to establish stations for magnetic and meteorological observations: a movement which was partly successful, though not to the extent desired. On the subject of international systems of observation and register, he justly remarks at a later date: "In order that the science of meteorology may be founded on reliable data, and attain that rank which its importance demands, it is necessary that extended systems of co-operation should be established. In regard to climate, no part of the world is isolated: that of the smallest island in the Pacific, is governed by the general currents of the air and the waters of the ocean. To fully understand therefore the causes which influence the climate of any one country, or any one place, it will be necessary to study the conditions, as to heat, moisture, and the movements of the air, of all others. It is evident also that as far as possible, one method should be adopted, and that instruments affording the same indications under the same conditions should be employed. . . . A general plan of this kind, for observing the meteorological and magnetical changes, more extensively than had ever before been projected, was digested by the British Association

* Sill. *Am. Jour. Sci.* April 1832, vol. xxii. pp. 150–155.

in 1838, in which the principal governments of Europe were induced to take an active part; and bad that of the United States, and those of South America, joined in the enterprise, a series of watch-towers of nature would have been distributed over every part of the earth." *

A large collection of original notes of various meteorological observations,—on magnetic variations, on auroras with attempts at ascertaining their extreme height, on violent whirl-winds, on hail-stones, on thunder-storms, and the deportment of lightning-rods,—unfortunately never published nor transcribed were lost (with much other precious scientific material) by a fire in 1865. The phenomena of thunder-storms were always studied by Henry with great interest and attention. A very severe one which visited Princeton on the evening of July 14th 1841, was minutely described in a communication to the American Philosophical Society, November 5th, 1841.†

On November 3d, 1843, he made a communication to the Society "in regard to the application of Melloni's thermo-electric apparatus to meteorological purposes, and explained a modification of the parts connected with the pile, to which he had been led in the course of his researches. He had found the vapors near the horizon, powerful reflectors of heat; but in the case of a distant thunder-storm, he had found that the cloud was colder than the adjacent blue space."‡

On June 20, 1845, he read a paper before the Society on "a simple method of protecting from lightning, buildings covered with metallic roofs;" urging the importance in such cases of having the vertical rain pipes always in good electrical connection with the earth, since "on the principle of electrical induction, houses thus covered are evidently more liable to be struck than those furnished either with shingle or tile." It is of course necessary to have the metallic roof in good metallic connection with the gutters and pipes; and the latter may conveniently have soldered to the lower end a ribbon of sheet copper two or three inches wide, continuing into the ground surrounded with charcoal and extending out from the house till it terminates in moist ground.§

In this paper he incidentally meets the much debated question whether a lightning-rod is efficient as a conductor by its solidity,

* *Agricultural Report* of Com. of Patents, for 1855, p. 367.

† *Proceed. Am. Phil. Soc.* vol. ii. pp. 111–116.

‡ *Proceed. Am. Phil. Soc.* vol. iv. p. 22.

§ *Proceed. Am. Phil. Soc.* vol. iv. p. 179. Henry appears to have been much impressed with the conducting value of the tinned sheet-iron pipes commonly used as rain spouts, from observing that amid the strange vagaries of the circuitous path pursued by the lightning (in cases of houses struck by this destructive agent), the rain pipe was not unfrequently selected as part of the route;—marks of explosive violence being exhibited at its lower end, and sometimes at its top as well.

or by its surface only. While he had been able to magnetize small needles placed transversely to the edges of broad strips of copper, through which electrical discharges were passed, he could obtain no signs of magnetism in needles when placed transversely near the sides of such strips about mid-way from the edges. In like manner he failed to discover any action in a small magnetizing helix placed within a section of gas-pipe and connected with it at either end, when transmitting through the system an electrical spark; while he easily obtained magnetic effects with a galvanic current passed through the same arrangement.* From these and other experiments he was led to believe that mechanical electricity tends to pass mainly along the exterior surface of a conductor, and accordingly that Ohm's law of conduction is not applicable to mechanical electricity.†

Some popular uneasiness having been excited in 1846, in consequence of telegraph poles being occasionally struck by lightning, and of the supposed danger to travellers along highways likely to result therefrom, a communication on the subject addressed to Dr. Patterson, one of the Vice-Presidents of the American Philosophical Society, was read before the Society, and referred to Professor Henry for report. This was in the very infancy of the electro-magnetic telegraph; as it had not then been in existence more than a couple of years. Henry responded in a communication read June 19th, 1846, to the effect that while telegraph wires as long conductors were eminently liable to receive discharges of atmospheric electricity both from charged clouds and from the varying electrical condition of the air at distant points along the line (as for example even by a fog or precipitation of vapor at one station) as also from induction at a distance, the danger to travellers along a telegraph road would be very slight, unless a person should be standing or passing quite close to a pole at the moment of its being struck. He however recommended that for the protection of the poles, they should be provided with conductors. "The effects of powerful discharges from the clouds may be prevented in a great degree by erecting at intervals along the line and beside the supporting poles a metallic wire connected with the earth at the lower end, and terminating above at the distance of about half an inch from the wire of the telegraph. By this

* In passing a galvanic current through an iron tube, he obtained the evidence of an induction from both the inside and the outside of the tube, but in opposite directions.

† This very important question cannot be regarded as even yet decisively settled:—eminent authorities maintaining that electricity *in flow*—of whatever origin—observes equally the ratio of proportionality to area of cross section in the conductor. Probably the law of conductivity varies with circumstances.

arrangement, the insulation of the conductor will not be interfered with, while the greater portion of the charge will be drawn off. I think this precaution of great importance at places where the line crosses a river and is supported on high poles. Also in the vicinity of the office of the telegraph, where a dischage falling on the wire near the station might send a current into the house of sufficient quantity to produce serious accidents."* This precaution has now been largely adopted, especially on the telegraph lines of the central portion of the United States, which are more liable to the effects of lightning.†

Molecular Physics.—Among other inquiries many original examinations were made by Henry in the domain of molecular physics. While Professor in the College of New Jersey in 1839, his attention was attracted to a curious case of metallic capillarity. A small lead tube about eight inches long happening to be left with a bent end lying in a shallow dish of mercury, he noticed a few days afterward that the mercury had disappeared from the dish, and was spread on the shelf about the other end of the tube. On a careful examination of the tube by incision, it appeared that the mercury had not passed along the open canal of the tube, but had percolated through its solid substance. To test this, a solid rod of lead about one-fourth of an inch thick and seven inches long was bent into a siphon form, and the shorter end immersed in a small shallow vessel of mercury; a similar empty vessel being placed under the longer end. In the course of 24 hours a globule of mercury was found at the lower end of the lead rod; and in five or six days it had all passed over excepting what appeared in the form of crystals of a lead amalgam in the upper vessel.‡ A long piece of thick lead wire was afterward suspended in a vertical position, with its lower end dipping into a cup of mercury. In the course of a few days, traces of the mercury were found in the rod at the height of three feet above the cup: thus showing that a metal impervious to water or oil (excepting under very great pressure) was easily penetrated to great distances by a liquid metal.

Some years later on a visit to Philadelphia he endeavored with the assistance of his friend Dr. Patterson (then Director of the United States Mint), by melting a small globule of gold on a plate of clean sheet-iron, to obtain its capillary absorption; but without effect; probably owing to the interposition of a thin film of oxide. Applying to another personal friend, Mr. Cornelius of Philadelphia, a very intelligent and ingenious manufacturer of bronzes, and plated ornaments for chandeliers, etc. to try

* *Proceed. Am. Phil. Soc.*, vol. iv. p. 268.

† Prescott. *Electricity and the Electric Telegraph*, 8vo. N. York, 1877, chap. xxiii. pp. 296, and 411.

‡ *Proceed. Am. Phil. Soc.*, vol. i. p. 82.

whether a piece of silver-plated copper heated to the melting point of silver would show any absorption of that metal, he learned that it was a common experience under such circumstances to find the silver disappear; but that this had always been attributed to a volatilization of the silver, or in the workman's phrase,—to its being "burnt off." At Henry's request the experiment was tried: the heated end of a silver-plated piece of copper exhibited on cooling and cleaning, a copper surface, the other end remaining unchanged. Henry next had the copper surface slightly dissolved off by immersion for a few minutes in a solution of muriate of zinc, when as he had anticipated, the silver was again exposed, having penetrated to but a very short and tolerably uniform distance below the original surface.*

In 1844, he made some important observations on the cohesion of liquids. Notwithstanding that Dr. Young early in the century maintained that "the immediate cause of solidity as distinguished from liquidity is the *lateral adhesion* of the particles to each other," and had shown that "the resistance of ice to extension or compression is found by experiment to differ very little from that of water contained in a vessel,"† all the most popular text-books on physics continued to teach that the cohesion of the liquid state is intermediate between that of the solid and the gaseous states.‡ It seemed therefore desirable to test the question by some more direct means than the resistance of liquids contained in closed vessels; and for this purpose Henry employed the classical soap-bubble. "The effect of dissolving the soap in the water is not as might at first appear, to increase the molecular attraction, but to diminish the mobility of the molecules." In fact the actual tenacity of pure water is greater than that of soap-water.

The first set of experiments was directed to determine "the quantity of water which adhered to a bubble just before it burst." The second set of experiments was devised to measure the contractile force of a soap-bubble blown on the wider end of a U shaped glass tube half filled with water, by the barometric column sustained in the narrower stem of the tube; the difference of level being carefully observed by means of a microscope. The thickness of the soap-bubble film at its top was estimated by the

* *Proceed. Am. Phil. Soc.* June 20, 1845, vol. iv. p. 177.

† Young's *Lectures on Nat. Philos.* Lect. 50, vol. i. p. 627.

‡ "If we attempt to draw up from the surface of water a circular disk of metal say of an inch in diameter, we shall see that the water will adhere and be supported several lines above the general surface. This experiment which is frequently given in elementary books as a measure of the feeble attraction of water for itself, is improperly interpreted. It merely indicates the force of attraction of a single film of atoms around the perpendicular surface, and not of the whole column elevated." (*Agricultural Report* for 1857, p. 427.—Paper on Meteorology.)

last of the Newton rings shown previous to bursting. The result arrived at from both sets of experiments was that water instead of having a cohesion of 53 grains to the square inch (as was very commonly stated), has a cohesive force of several hundred pounds to the inch; or that the intermolecular cohesion of a liquid is fully equal to that of the substance in the solid state.*

In 1846, he presented to the Philosophical Society an epitome of his views on the molecular constitution of matter; giving the reasons for accepting the atomic hypothesis of Newton. He pointed out that the discovery and establishment of a general scientific principle "is in almost all cases the result of deductions from a rational antecedent hypothesis, the product of the imagination; founded it is true on a clear analogy with modes of physical action, the truth of which have been established by previous investigation:" and he urged that the hope of further advancement lies in the assumption "that the same laws of force and motion which govern the phenomena of the action of matter in masses, pertains to the minutest atoms of these masses." He therefore felt "obliged to assume the existence of an ætherial medium formed of atoms which are endowed with precisely the same properties as those we have assigned to common matter."

"According to the foregoing rules we may assume with Newton, the existence of *one kind of matter* diffused throughout all space, and existing in four states, namely the ætherial, the aeriform, the liquid, and the solid."† In referring to this postulated *fourfold state of matter*, Henry was accustomed to point out the remarkable analogy between this conception, and that of the four elements of the ancients,—fire, air, water, and earth.

"In conclusion, it should be remembered that the legitimate use of speculations of this kind, is not to furnish plausible expla-

Proceed. Am. Phil. Soc. April 5, and May 17, 1844, pp. 56, 57, and 84, 85. The original notes of these interesting experiments containing the numerical results obtained under a great variety of conditions, laid aside for further reductions and comparisons, were destroyed by fire in 1865. Since the density of most solid substances differs very slightly from that of their liquid state, being indeed less in many,—unless at considerably lower temperatures, (as in the case of ice, and most of the metals.) it appears quite improbable that the difference between solidity and liquidity could depend in any case on the degree of cohesion. On the contrary, the cohesion of water should be sensibly greater than that of ice, since its constituent molecules are closer together. Of the nature of that "lateral *adhesion*" which resists the flow of solids (excepting under the conditions of great strain—long continued), and whose absence is marked in liquids by their almost perfect and frictionless mobility, our present science affords us no intimation.

† Two hundred years ago, Newton speculating on the unity of matter, ventured the suggestion, "Thus perhaps may all things be originated from æther."—Letter to the Secretary of the Royal Society—Henry Oldenburg, January, 1676 (*Hist. of Roy. Soc.* by Thomas Birch, vol. iii. p. 250).

nations of known phenomena, or to present old knowledge in a new and more imposing dress, but to serve the higher purpose of suggesting new experiments and new phenomena, and thus to assist in enlarging the bounds of science, and extending the power of mind over matter; and unless the hypothesis can be employed in this way, however much ingenuity may have been expended in its construction, it can only be considered as a scientific romance worse than useless, since it tends to satisfy the mind with the semblance of truth, and thus to render truth itself less an object of desire."*

Light and Heat.—Henry also made important investigations on some peculiar phenomena connected with light and heat. For the purpose of experimenting on sun light he devised in 1840, a very simple form of heliostat, based on the suggestion of Dr. Young, whereby the solar ray was received into an upper room in a direction parallel to the earth's axis, requiring therefore only an equatorial movement of the reflector; which was effected by the aid of a common cheap pocket watch placed on a small hinged board set by a screw to the angle of latitude. The mirror mounted on a swivel and properly balanced, presented no sensible resistance to the running of the watch, which was arranged for the 24 hour rotation by a watch-maker of Princeton. The whole cost of the completed instrument (including the time-movement) was but sixteen dollars. If any particular direction of the ray was required, it was only necessary to place a stationary mirror in the fixed path of the ray, adjusted to the desired angle.†

In 1841, on repeating experiments of Becquerel and Biot on "Phosphorescence," he discovered some new characteristics in the emanation (particularly when excited by electrical light) which had not before been observed.‡ These were more fully detailed in a communication made to the American Philosophical Society, in 1843, "On Phosphorogenic Emanation." This phenomenon had been first observed in the diamond, when taken into a dark room immediately after exposure to direct sun-light, or to a vivid electric spark; and was afterward observed in several other substances,—notably in the chloride of calcium—"Homberg's phosphorus."§ It had also been shown by Becquerel that

* *Proceed. Am. Phil. Soc.* Nov. 6, 1846, vol. iv. pp. 287-290.

† *Proceed. Am. Phil. Soc.* Sept. 17, 1841, vol. ii. p. 97.

‡ *Proceed. Am. Phil. Soc.* April 16, 1841, vol ii. p. 46.

§ Homberg's phosphorus is chloride of calcium prepared by melting one part of sal ammonia with two parts of slaked lime. Canton's phosphorus is sulphide of calcium formed by a mixture of three parts of sifted and calcined oyster shells, and one part of flowers of sulphur, exposed for an hour to a strong heat.

while this phosphorescence may be fully excited in the sensitive body by rays which have passed through transparent sulphate of lime, or through quartz, the effect is entirely arrested by a plate of transparent mica, or glass. Henry by a long series of experiments greatly extended these lists, including in them a large number of liquids. He also subjected both the exciting rays (especially of the electric spark) and the luminous emanation, to various treatment, by reflection, refraction, polarization, etc. The Nicol prism was found to obstruct this peculiar exciting ray so much as to permit scarcely any impression; but what was remarkable and unexpected, a pile of thin mica plates which seemed to cut off entirely the phosphorogenic impression, was found when placed obliquely at the best polarizing angle, to distinctly excite a surviving luminous spot. On examination of the phosphorescence excited by polarized light, no effect was produced by a rotation of the analyser: "when the beam was transmitted through crystals in different directions with reference to their optical axis, no difference could be observed." The phosphorescence was completely depolarized, as if taking an entirely new origin in the sensitive substance: a fact re-discovered by Professor Stokes some ten years later, with regard to fluorescent emanations.

That the phosphorogenic effect does not depend on a heating of the substance, appeared to be shown by the fact that "the lime becomes as luminous under a plate of alum as under a plate of rock-salt." The emanation was examined by a prism of rock-crystal, and by one of rock-salt:—science had not then the spectroscope. While the impression could be readily made by a reflected beam from a metallic mirror, it failed entirely when directed from a looking-glass. The luminous effect on the phosphorescent substance was found to be defined in location by the form of the opening made in sheet metal screens. On testing with different portions of the electric spark by means of a narrow slit in the screen, the two terminals of the spark were found to be much more active (as measured by the subsequent duration of the phosphorescence) than the middle portion. By a suitable arrangement of double screens with three slits each, he was able to make simultaneous star-like photographs on the substance, of the two extreme portions of the spark and of a middle point: and while the latter point "exhibited a feeble phosphorescence for two or three seconds" only, the two former "continued to glow for more than a minute:" and yet the middle of the spark appeared to the eye quite as vivid as its extremities. It was also observed that while a sensitive daguerreotype plate received no impression from the electric spark, inversely another similar plate exposed for several minutes to the direct light of the full

moon received a photographic impression, while the lime similarly exposed, exhibited no phosphorescence.*

Henry was afterward accustomed on the occurrence of a bright aurora, to expose a sheet of paper written or figured with a solution of bisulphate of quinia to the auroral light, when the characters (quite invisible by lamp-light or even by day-light) would distinctly glow with a pale blue light;—indicating the electrical nature of the meteor.

In January, 1845, in conjunction with Professor Stephen Alexander, he instituted a series of experimental observations on the relative heat-radiating power of the solar spots. On the 4th of January a large spot through which our terrestrial globe could have been freely dropped, (having been estimated at more than 10,000 miles in diameter,) favorably situated near the middle of the disk, was examined with a telescope of four inches aperture. A screen having been arranged in a dark room, with a thermo-electric apparatus behind it and having its terminal or pile just projecting through a hole in the screen, the image of the spot was received upon it, giving a clearly defined outline about two inches long and one inch and a half wide. By a slight motion of the telescope the spot could readily be thrown on or off the end of the pile as desired. A considerable number of observations indicated very clearly by the differing deflections of the galvanometer needle "that the spot emitted less heat than the surrounding parts of the luminous disk."† A brief account of the results obtained by these researches given in a letter to his friend Sir David Brewster, was read by him at the Cambridge Meeting of the British Association in June, 1845.‡ The determinations arrived at have been fully confirmed by the later observations of Secchi and others §

In 1845, he contributed a paper to the Princeton Review, on "Color Blindness;" which although in the modest form of a literary review of two Memoirs then recently published, (that of Sir David Brewster in the Philosophical Magazine; and that of

* *Proceed. Am. Phil. Soc.* May 26, 1843, vol. iii. pp. 38–44. This interesting but obscure subject although apparently connected with the phenomenon of "fluorescence" has yet an entirely distinct phase in its abnormal continuance of luminosity,—similar to the familiar effect of a thermal impression. It is possible however that the conversion of wave-periodicity (wave-length), shown by Stokes to be the characteristic of fluorescence, may require time for its full development.

† *Proceed. Am. Phil. Soc.* June 20, 1845, vol. iv. p. 176.

‡ *Report Brit. Assoc.* 1846, part ii. p. 6.

§ P. Angelo Secchi—during the years 1848, and 1849, was Professor of Mathematics at the College of Georgetown, D. C.: and in the preparation of his "Researches on Electrical Rheometry" published in the third volume of the *Smithsonian Contributions*, (art. ii. p. 60,) he received from Henry the friendly assistance of apparatus and suggestions. He was appointed Director of the Observatory at Rome, in 1850.

Professor Elie Wartman, of Lausanne, in the Scientific Memoirs,) supplied original observations on this interesting department of the physiology of vision.

Miscellaneous Contributions.—Henry's miscellaneous contributions to physical science are so numerous and varied, that only a brief allusion to some of them can be afforded. In 1829, he published quite an elaborate "Topographical sketch of the State of New York, designed chiefly to show the general elevations and depressions of its surface."* And in later years he devoted much attention to physical geography. He performed at various times, a good deal of chemical work (chiefly of an analytical character),—first as Dr. T. Romeyn Beck's assistant,† and afterward independently, as well as mediately in directing his own pupils and assistants. In 1833, he devised an improvement of Wollaston's mechanical scale of the chemical equivalents, for the benefit of his pupils in chemistry:—a contrivance which was much used and highly appreciated at the time.

The suggestion had been thrown out by more than one astronomer, that carefully timed observations on characteristic meteors or "shooting-stars" might be made available for determining differences of longitude between the stations of observation.‡ For many years however the proposition had been generally regarded as offering rather a speculative than a practical method of solving a problem of so great nicety. Henry in concert with his brother-in-law, Professor Alexander, and with his friend Professor Bache, determined to ascertain by actual trial the availability and value of the system. On the 25th of November, 1835, Professor Bache observing at his residence in Philadelphia (assisted by Professor J. P. Espy,)—simultaneously with Professor Henry and Professor Alexander, at the Philosophical Hall at Princeton, they obtained seven co-incidences:—the instant of disappearance of the meteor being in each case selected as the most accurately attainable epoch. These seven observations (whose greatest discrepancies amounted to but a trifle over 3 seconds) gave a mean result of 2 minutes 0.61 second (time longitude) differing only

* *Trans. Albany Institute*, vol. i. pp. 87–112.

† "Henry was then Dr. Beck's chemical assistant, and already an admirable experimentalist." Address before the Albany Institute, by Dr. O. Meads, May 25, 1871. (*Trans. Albany Instit.* vol. vii. p. 21.)

‡ "The merit of first suggesting the use of shooting stars and fire-balls as signals for the determination of longitudes is claimed by Dr. Olbers and the German astronomers for Benzenberg, who published a work on the subject in 1802. Mr. Bailey however has pointed out a paper published by Dr. Maskelyne twenty years previously, in which that illustrious astronomer calls attention to the subject, and distinctly points out this application of the phenomena." This was dated Greenwich, November 6th, 1783. (*L. E. D. Phil. Mag.* 1841, vol. xix. p. 554.)

one second and two-tenths from the mean estimate of relative longitude arrived at by other methods.*

In 1840, Henry gave an account of "electricity obtained from a small ball partly filled with water, and heated by a lamp."†

In 1843, he read a communication to the Society, "On a new method of determining the velocity of Projectiles:" for this purpose employing two screens of fine insulated wire each in circuit with a galvanometer, and at determined near distances in the path of the projectile;—whereby the galvanic currents would be successively interrupted at the instants of penetration. To record the interval, each galvanometer needle is provided at one end with a marking pen touching a horizontally revolving cylinder, which is divided by longitudinal lines into 100 equal parts, and is driven by clock-work at the rate of ten revolutions per second, giving therefore to the interval of passage between two consecutive lines, the thousandth part of a second.‡ Another still more ingenious method is suggested, whereby the galvanometer may be dispensed with: each circuit including an induction coil, one end of whose secondary circuit is connected with the axis, and the other end placed very nearly in contact with the surface of the graduated paper on the revolving cylinder, so as to give the induction spark through the paper at the instant of the interruption of the primary circuits by the projectile passing through the wire screens. This is really a much neater and more direct application of the electric interruption than the employment of a galvanometer needle for making the record. If desirable, the cylinder may be made to have a very slow longitudinal movement by a screw, so as to give a helical direction to the tracings; and different pairs of screens similarly arranged at distant points in the path of the projectile may be employed to determine the variations of velocity in its flight.§

Henry was always a watchful student of psychological and subjective phenomena. Witnessing on one occasion the performance of an athlete before a large assembly, he noticed with a curious interest the "inductive" sympathy manifested by nearly

* *Proceed. Am. Phil. Soc.* Dec. 20, 1839, vol. i. pp. 162, 163. "This appears to have been the first actual determination of a difference of longitude by meteoric observations." (*L. E. D. Phil. Mag.* 1841, vol. xix. p. 553.) Several years later (in 1838) similar meteoric observations were made between Altona and Breslau; and also between Rome and Naples.

† *Proceed. Am. Phil. Soc.* Dec. 18, 1840, vol. i. p. 323.

‡ It appears that Wheatstone devised his ingenious electro-magnetic "chronoscope" in 1840; though he unfortunately published no account of it till 1845; or two years after the publication by Henry. And this was called out as a reclamation, on the publication of a similar invention by L. Breguet, of Paris, in January of the same year.

§ *Proceed. Am. Phil. Soc.* May 30, 1843, vol. iii. pp. 165-167.

every spectator (himself included) in being swayed by a movement as of assistance to the performer. In remarking the impression of being moved, while steadily watching a series of passing canal boats,—on each boat reaching a certain point, he referred the impression (amounting almost to a sensation of movement), to the relative angle of vision formed by the moving body.

He made a number of experiments on the flow of water jets under varying conditions: also observations on sonorous flames when passing into a stove-pipe of eight inches diameter and about ten feet in length: on the comparative rates of evaporation from fresh and from salt water: on the slow evaporation of water from the end of a tube, and the much greater rapidity of evaporation when the tube is open at both ends: extended notes of which, with a great number of other researches, perished in the flames.

In 1844, he published a Syllabus of his Lectures at Princeton. In December of that year he presented to the Philosophical Society a communication of a somewhat more theoretical character than usual,—on the derivation and classification of mechanical motors. He refers these to two classes;—the first, those derived from celestial disturbance (as water, tide, and wind powers),—and the second, those derived from organic bodies or forces (as steam and other heat powers, and animal powers). The forces of gravity, cohesion, and chemical affinity are not included, since these tend speedily to stable equilibrium; and they become sources of mechanical power only as they are disturbed by some of those before mentioned. It is not the running down of the water-fall, or the clock-weight, which is the true origin of their useful work, but the lifting of them up. The same is true of the power derived from combustion. He then adds that his second class (the forces derived from the organic world) might perhaps by a similar process of reasoning be derived from the first class; (that of celestial disturbance;)—regarding "animal power as referable to the same sources as that from the combustion of fuel," and the action of the vegetative power as "a force derived from the divellent power of the sunbeam," being simply a case of solar de-oxidation. Organism—vegetable and animal, he considers as built up under the *direction* of a vital principle, which is not itself a mechanical force. Volcanic power is neglected as comparatively feeble and limited, and not practically utilized.*

* *Proceed. Am. Phil. Soc.* Dec. 20, 1844, vol. iv. pp. 127–129. This appears to be the first—as it is probably the best—analysis of physical energy, which has been proposed. Twenty years later, a similar analysis (with certainly no improvement in the classification) was adopted by Prof. Tait, in an essay on "Energy;" (*North British Review*, 1864, vol. xl. art. iii. p. 191, of Am. edition:) and by Dr. Balfour Stewart, in his *Elementary Treatise on Heat*, Oxford, 1866 (book iii. chap. v. art. 388, p. 354.)

This interesting digest presents one of the earliest and clearest theoretical statements we have, of the correlation and transformation of the physical forces; including with these the so-called organic forces.

ADMINISTRATION OF THE SMITHSONIAN INSTITUTION.

By an Act of Congress approved August 10, 1846, the liberal bequest to the United States, for the promotion of Science, by James Smithson of London, England, was appropriated to the foundation of the Institution bearing his name; the establishment being made to comprise the chief dignitaries of the Government as the supervising body, and a Board of Regents being created for conducting the business of the Institution after completing its organization. As the testator had bequeathed his fortune,* in simple terms "for the increase and diffusion of knowledge among men," there arose not unnaturally a great diversity of opinion both among Congressmen, and among the Regents, as to the most desirable method of executing the purpose of the Will: and the organizing Act was itself a sort of compromise, after many years of discussion and disagreement in both branches of Congress. To literary men, no instrument of knowledge could be so important as an extensive Library:—to the professional, a seat of education or public instruction—general or special—supplemented by elaborate courses of public lectures, appeared the obvious and necessary means of diffusing useful learning,—to the "practical," a large agricultural and polytechnic institute—supplemented perhaps by a museum, was the only fitting plan of developing the resources of our country:—to the artistic, extensive galleries of art were the most worthy and instructive objects of patronage. The Regents sought counsel from the distinguished and the learned: and several of them applied to Professor Henry for his opinion. He gave the subject a careful consideration; and announced very decided views. As Smithson was a man of scientific culture, a Fellow of the Royal Society, an expert analytical chemist, and devoted to original research, Henry held that the language of his Will must receive its most accurate and scientific and at the same time most comprehensive interpretation; that the words "increase and diffusion of knowledge among men" were deliberately and intelligently employed; and that no local or even national interests were as broad as its terms,—that no merely educational projects of whatever character, no schemes of material and practical advancement however useful, could justly be regarded as fulfilling the obvious intent—expressed by a scientific thinker

* The whole amount of the bequest was a trifle over 100,000 pounds, or about 540,000 dollars.

and writer—first of all the *increase* of knowledge by the promotion of original research,—the addition of new truths to the existing stock of knowledge, and secondly—its widest possible diffusion among mankind.*

These wise and far-reaching views exerted a marked influence; and though hardly then in accord with the opinion of the majority, yet led to his election December 3d, 1846, as the "Secretary" and practical Director of the infant institution. A second time was Henry called upon to sever dearly prized associations,—the prosperous and congenial pursuits of fourteen years within the classic halls of Princeton. One motive turned the wavering scale. Here was a rare occasion offered by the enlightened provision of James Smithson, to secure for abstract science and unpromising original research, a much needed encouragement and support; and an obligation upon the scientific few to resist and if possible prevent the perversion of the trust to the merely popular uses of the short-sighted many. That years would be required for shaping the character and conduct of the institution as he desired, was certain;—that this could not be effected without much opposition and various obstacles, he very clearly foresaw. That during these years of active supervision and direction, he must abandon all hope of personal opportunity for original research, he as freely accepted in the expressive remark made to a trusted friend in consultation on the occasion: "If I go, I shall probably exchange permanent fame for transient reputation."

With the assurance of the Trustees of the College of New Jersey, that should he fail to realize his programme, or should he satisfactorily accomplish his apostolic purpose, his chair should always be at his command, with a hearty welcome back, Henry, neither spurred by over-confidence, nor depressed with undue timidity, though filled with anxious solicitude for the future, accepted the appointment tendered to him. He removed with his family to Washington, and at once commenced his administration of the duties assigned to him by the Regents of the Institution.

Summoned thus to the occupancy of a new and untried field, and to the discharge of essentially executive functions, he from the first displayed a clearness and promptness of judgment, a singleness and steadiness of aim, a firmness and consistency of decision, combined with a practical sagacity and moderation in adapting his course to the exigencies of adverse conditions, which stamped him as a most able and successful administrator. Without concealment and without diplomacy, his distinctly avowed principle of action was steadily and patiently pursued. With

* "Programme of Organization." Smithsonian Report for 1847.

honest submission to the controlling Act of Congress, he made as honest avowal of his desire and of his endeavor to have that legislation modified. Hampered by provisions he deemed unwise and injurious, he yet skilfully managed to reconcile contestant interests, and to secure the entire confidence and concurrence of the Regents. Henceforth his purpose and his effort were to be directed to the unique object of encouraging and fostering the development of what has so flippantly been designated "useless knowledge;" and merging self in the community of physical inquirers and collaborators, to become the high-priest of abstract investigation;—prepared to lend all practicable assistance to that small but earnest band of nature-students, who inspired by no aims of material utility, seek from their mistress as the only reward of their devotion, the higher knowledge of truth.*

Of the two distinct objects of endowment specified by Smithson's Will,—"the *increase*—and the *diffusion*—of knowledge," Henry forcibly remarked: "These though frequently confounded, are very different processes, and each may exist independent of the other. While we rejoice that in our country above all others, so much attention is paid to the *diffusion* of knowledge, truth compels us to say that comparatively little encouragement is given to its *increase*.† There is another division with regard to knowledge which Smithson does not embrace in his design; viz. the application of knowledge to useful purposes in the arts. And it was not necessary he should found an institution for this purpose. There are already in every civilized country, establishments and patent laws for the encouragement of this department of mental industry. As soon as any branch of science can be brought to bear on the necessi-

* Henry has finely said: "Let censure or ridicule fall elsewhere,—on those whose lives are passed without labor and without object; but let praise and honor be bestowed on him who seeks with unwearied patience to develop the order, harmony, and beauty of even the smallest part of God's creation. A life devoted exclusively to the study of a single insect, is not spent in vain. No animal however insignificant is isolated; it forms a part of the great system of nature, and is governed by the same general laws which control the most prominent beings of the organic world." (*Smithsonian Report* for 1855, p. 20.)

† Swainson the Naturalist, the countryman and friend of Smithson, has very pointedly marked this recognized distinction. "The constitution of the Zoological Society is of a very mixed nature, admirably adapted indeed to the reigning taste. It is more calculated however to *diffuse* than to *increase* the actual stock of scientific knowledge." (*Discourse on the Study of Natural History*, Cabinet Cyclopædia, 16mo. London, 1834, part iv. chap. i. sec. 221, p. 314.) And again: "It is very essential when we speak of the diffusion or extension of science, that we do not confound these stages of development with discovery or advancement; since the latter may be as different from the former as depth is from shallowness." (Same work, part iv. chap. ii. sec. 240, p. 343.)

ties, conveniences, or luxuries of life, it meets with encouragement and reward. Not so with the discovery of the incipient principles of science. The investigations which lead to these, receive no fostering care from Government, and are considered by the superficial observer as trifles unworthy the attention of those who place the supreme good in that which immediately administers to the physical needs or luxuries of life. If physical well-being were alone the object of existence, every avenue of enjoyment should be explored to its utmost extent. But he who loves truth for its own sake, feels that its highest claims are lowered and its moral influence marred by being continually summoned to the bar of immediate and palpable utility. Smithson himself had no such narrow views.* The prominent design of his bequest is the promotion of abstract science. In this respect the Institution holds an otherwise unoccupied place in this country; and it adopts two fundamental maxims in its policy;—first to do nothing with its funds which can be equally well done by other means; and second to produce results which as far as possible will benefit mankind in general." †

Congress—naturally with a prevailing tendency to the literary, the showy, and the popular, had (after eight years of dilatory controversy) directed in its organizing Act (sec. 5,) the erection of a building "of sufficient size, and with suitable rooms or halls for the reception and arrangement upon a liberal scale, of objects of natural history including a geological and mineralogical cabinet, also a chemical laboratory, a library, a gallery of art, and the necessary lecture-rooms." By the 9th section of the Act, the Board of Regents were authorized to expend the remaining income of the endowment "as they shall deem best suited for the promotion of the purpose of the testator." Out of an annual income of some 40,000 dollars, the Regents in full accord with their Secretary (whose carefully elaborated programme they officially adopted December 13, 1847,) succeeded in creditably inaugurating all the objects specified in the charter; and at the same time in establishing the system of publication of original Memoirs, to which Henry justly attached the first importance.

An incident in itself too slight to produce a visible ripple on the current of Henry's life, is yet too characteristic to be here

* In regard to the value of scientific truth, Smithson in a communication dated June 10th 1824, has forcibly expressed his strong "conviction that it is in his *knowledge* that man has found his greatness and his happiness, the high superiority which he holds over the other animals who inhabit the earth with him; and consequently that no ignorance is probably without loss to him, no error without evil." (*Thomson's Annals of Philosophy*, 1824, vol. xxiv. or new series vol. viii. p. 50.)

† *Smithsonian Report* for 1853, p. 8.

omitted. Dr. Robert Hare having in 1847 decided upon resigning his Professorship of Chemistry in the Medical Department of the University of Pennsylvania, (the largest and best patronized in the country,) the vacant chair was tendered by the Board of Trustees to Professor Henry. His friend Dr. Hare himself used his influence to induce Henry to become his successor; particularly dwelling on the large amount of leisure afforded for independent investigations. The income of this professorship was more than double the salary of the Smithsonian Secretaryship. The position tempting as it might have been under different circumstances, was however declined. Henry felt that to leave his present post before his cherished policy was fairly settled and established, would be most probably to abandon nearly all the results of the experiment: and having set before himself the one great object of directing the resources of the Smithsonian Institution as far as possible to the advancement of science, in conformity with the undoubted intention of its founder, (and as the execution therefore of a sacred trust,) he resolutely put aside every inducement that might divert him from the fulfillment of his task.*

Of the half a dozen objects of attention specified in the 5th section of the organizing Act, (the various inspiration of different partisans,) not one directly tended to further the primary requirement of the Will :—even the Laboratory being avowedly introduced simply as a utilitarian workshop for mining and agricultural analysis. Regarded as methods of *diffusing* existing knowledge they were obviously local and limited in their range: and as compared with the instrumentality of the Press, were certainly very inefficient for spreading the benefits of the endowment among men.†

* Some six years later, a somewhat similar temptation was presented. In 1853, on the resignation of President Carnahan of the College of New Jersey at Princeton, an effort was made to induce the return of Professor Henry to his academic seat, by a movement to obtain for him the Presidency of the College. Such a token of affectionate remembrance could not but be grateful and touching to his feelings; but a sense of obligation was upon him, not to be laid aside. He had undertaken a work and a responsibility which must not be left to the hazard of failure. He declined the proffered honor—with thanks; and warmly recommended Dr. Maclean to the vacant position: who thereupon was duly elected. (Maclean's *Hist. of College of New Jersey*, vol ii. p. 336.)

† "The objects specified in the Act of Congress evidently do not come up to the idea of the testator as deduced from a critical examination of his will. A library, a museum, a gallery of arts, though important in themselves, are local in their influence. I have from the beginning advocated this opinion on all occasions, and shall continue to advocate it whenever a suitable opportunity occurs." (*Smithsonian Report* for 1853, p. 122 (of Senate edit.) p. 117. (of H. Rep. edit.) The superficial pretext was not wanting on the part of some, that the words "increase and diffusion" were not to be taken too literally, but to be considered as the

Henry with a rare courage dared maintain against most powerful influence, that the interests specifically designated must all be subordinated to the fundamental requirement, the promotion of original research for increasing knowledge; and that this was amply sustained by the residuary grant of authority to the Regents (under the 9th section of the Act) "to make such disposal as they shall deem best suited *for the promotion of the purposes of the testator*, anything herein contained to the contrary notwithstanding," of any income of the Smithsonian fund "not herein appropriated, or not required for the purposes herein provided." Henry's carefully studied programme comprised two sections: the first, embracing the details of the plan for carrying out the explicit purpose of Smithson; the second, indicating the proper steps for carrying out the provisions of the Act of Congress. The first and principal section proposed as methods of promoting research,—the stimulation of particular investigations by special premiums,—the publication of such original memoirs furnishing positive additions to knowledge by experiment and observation as should be approved by a commission of experts in each case,—the active direction of certain investigations by the provision of instruments as well as of the necessary means, the appropriations being judiciously varied in distribution from year to year,—the prosecution of experimental determinations and the solution of physical problems,—the extension of ethnology (especially American) and in general the conduct of such varied explorations as should ultimately result in a complete physical atlas of the United States. As methods of promoting the diffusion of knowledge, it was proposed to give a wide circulation to the published original memoirs or Smithsonian "Contributions to Knowledge" among domestic and foreign libraries, institutions, and scientific correspondents, to have prepared by qualified collaborators, series of careful reports on the latest progress of science in different departments, and to provide facilities for the distribution and exchange of scientific memoirs generally.

It is unnecessary here to follow closely the slow steps by which —through all the obstructions of narrow prejudice and ignorant misconstruction, of selfish interest and pretended philanthropy, of friendly remonstrance and hostile denunciation,—the policy originally marked out by the Secretary was with unwavering resolution and imperturbable equanimity steadily pursued, until it gained its assured success; the vindication and the unpretentious triumph of "the just man tenacious of purpose."

The most formidable of the specialist schemes both in Congress

tautology of legal equivalents, applicable to the development of the individual mind; since school-boys (if not the pundits) were evidently capable of an "increase" of knowledge.

and elsewhere, was that of the Library faction, which prosecuted with remarkable zeal and energy, threatened by the acknowledged ability of its leading advocates to control the action of the Regents, even to the neglect and abandonment of all the other interests indicated by the statute. In Henry's judgment the Institution should possess simply a working library,* an auxiliary for those engaged in scientific research, a repertory well supplied with the published Proceedings and Transactions of learned Societies, but which so far from aiming at an encyclopædic or a literary character, should be mainly supplementary to the large National Library already established at the Capital. "The idea ought never to be entertained that the portion of the limited income of the Smithsonian fund which can be devoted to the purchase of books will ever be sufficient to meet the wants of the American scholar. On the contrary it is the duty of this Institution to increase those wants by pointing out new fields for exploration, and by stimulating other researches than those which are now cultivated. It is a part of that duty to make the value of libraries more generally known, and their want in this country more generally felt."†

Processes of Divestment.—Henry's declaration that the moderate means at command were insufficient to support worthily either a Library, or a Museum, alone, was early justified. The Library though slowly formed of only really valuable scientific works, and this largely by exchanges with the Smithsonian publications,‡ in the course of a dozen years amounted to about 50,000 volumes: and the annual cost of binding, superintendence, and the constant enlargement of room and of cases, was becoming a serious tax upon the resources of the Institution. The propriety of transferring the custody of this valuable and rapidly increasing collection to the National Library established by Congress, was repeatedly urged upon the attention of that body:

* To carry on the operations of the first section a working library will be required, consisting of the past volumes of the transactions and proceedings of all the learned societies in every language. These are the original sources from which the most important principles of the positive knowledge of our day have been drawn." (*Smithsonian Report* for 1847, p. 139, of Sen. ed.: p. 131, of H. Rep. ed.)

† *Smithsonian Report* for 1858, p. 224, (of Sen. ed.) p. 216, (of H. Rep. ed.)

‡ "It is the intention of the Regents to render the Smithsonian library the most extensive and perfect collection of Transactions and scientific works in this country, and this it will be enabled to accomplish by means of its exchanges, which will furnish it with all the current journals and publications of societies, while the separate series may be completed in due time as opportunity and means may offer. The Institution has already more complete sets of Transactions of learned societies than are to be found in the oldest libraries in the United States." (*Smithsonian Report* for 1855, p. 29.)

and by an Act approved April 5th, 1866, such transfer was at last effected.

"Congress had presented to the Institution a portion of the public reservation on which the building is situated. In the planting of this with trees, nearly 10,000 dollars of the Smithson income were expended." Ultimately however opportunity was taken to have the Smithsonian park included in the general appropriation by the Government for improving the public grounds.

The courses of Lectures which were continued from their establishment in 1849, to 1863, were then abandoned. In conformity with the judicious policy entertained from the beginning not to consume unprofitably the limited means of the Institution by attempting to do what could be as well or better accomplished by other organizations, its herbarium comprising 30,000 botanical specimens and other allied objects, was transferred to the custody of the Agricultural Department. Its collection of anatomical and osteological specimens was transferred to the Army Medical Museum. And its Fine-Art collections were transferred to the custody of the "Art-Gallery" established at Washington (with a larger endowment than the whole Smithsonian fund) by the enlightened liberality of Mr. W. W. Corcoran.

Such were the successive processes by which much of the early and injudicious legislative work of organization, intended for popularising the activities of the Institution, was gradually undone; greatly to the dissatisfaction and foreboding of many of its well-meaning friends. "It should be recollected," said Henry, "that the Institution is not a popular establishment."*

The National Museum.—The last heritage of misdirected legislation—the National Museum, still remains in nominal connection with the Institution; although Congress has recognized the justice of making special provision for its custody by an annual appropriation ever since its establishment in 1842,—four years before the organization of the Smithsonian Institution. The Government collection of curiosities had accumulated from the contributions of the various exploring expeditions; and Henry from the first, had objected to receiving it as a donation, foreseeing that it would

* *Smithsonian Report* for 1876, p. 12. A distinguished politician, now many years deceased, (an influential Member of Congress—and *possible* statesman,) in the confidence of friendship pointed out with emphasis, how by a few judicious expedients—involving only a moderate reduction of the income of the Institution, golden opinions might be won from the press, and the Smithsonian really be made quite a "popular" establishment. Unseduced by these friendly suggestions of worldly wisdom. Henry astonished his adviser by the smiling assurance that his self-imposed mission and deliberate purpose was to prevent, as far as in him lay, precisely that consummation. Had the philosopher repudiated the "breath of his nostrils" he could not have been looked upon by the politician, as more hopelessly demented.

prove more than "the gift of an elephant."* In his first Report, he ventured to say: "It is hoped that in due time, other means may be found of establishing and supporting a general collection of objects of nature and art at the seat of the general government, with funds not derived from the Smithsonian bequest."† In his third annual Report he remarked: "The formation of a Museum of objects of nature and of art requires much caution. With a given income to be appropriated to the purpose, a time must come when the cost of keeping the objects will just equal the amount of the appropriation: after this no further increase can take place. Also, the tendency of an Institution of this kind unless guarded against, will be to expend its funds on a heterogeneous collection of objects of mere curiosity." Justly jealous of any dependence of the Institution, designed as a monument to its founder, upon the varying favors or caprices of a political government, or of any confusion between the National Museum, and its own special collections for scientific study rather than for popular display, he added: "If the Regents accept this Museum, it must be merged in the Smithsonian collections. It could not be the intention of Congress that an Institution founded by the liberality of a foreigner, and to which he has affixed his own name, should be charged with the keeping of a separate Museum, the property of the United States. . . The small portion of our funds which can be devoted to a museum may be better employed in collecting new objects, such as have not yet been studied, than in preserving those from which the harvest of discovery has already been fully gathered." Nor was he reconciled to the gift by the suggestion that a suitable appropriation would be granted by the National Government, for the expense of its custody. "This would be equally objectionable; since it would annually bring the Institution before Congress as a supplicant for government patronage."‡

In his Report for 1851, he forcibly stated in regard to the requirements of a general Museum, that "the whole income devoted to this object would be entirely inadequate:" and he strongly urged a National establishment of the Museum on a basis and a scale which should be an honor and a benefit to the people and their Capital city. "Though the formation of a general collection is neither within the means nor the province of the Institution, it is an object which ought to engage the at-

* His friend Prof. Silliman in a letter dated December 4th, 1847, wrote: "If it is within the views of the Government to bestow the National Museum upon the Smithsonian Institution, the very bequest would seem to draw after it an obligation to furnish the requisite accommodations without taxing the Smithsonian funds: otherwise the gift might be detrimental instead of beneficial."

† *Smithsonian Report*, 1847, p. 139, (Sen. ed.), p. 132, (H. Rep. ed.)

‡ *Smithsonian Report* for 1849, pp. 181, 182, (of Senate ed.) pp. 173, 174, (of H. Rep. ed.)

tention of Congress. A general Museum appears to be a necessary establishment at the seat of government of every civilized nation. . . . An establishment of this kind can only be supported by government; and the proposition ought never to be encouraged of putting this duty on the limited though liberal bequest of a foreigner."* This project was urged in almost every subsequent Report. "There can be but little doubt that in due time ample provision will be made for a Library and Museum at the Capital of this Union, worthy of a government whose perpetuity depends upon the virtue and intelligence of the people. It is therefore unwise to hamper the more important objects of this Institution by attempting to anticipate results which will be eventually produced without the expenditure of its means."† "The importance of a collection at the seat of government, to illustrate the physical geography, natural history, and ethnology, of the United States, cannot be too highly estimated: but the support of such a collection ought not to be a burden upon the Smithsonian fund."‡

The popular mind did not however appear to be prepared to accept these earnest presentations; and in 1858, the National Museum was transferred by law to the custody of the Smithsonian Institution, with the same annual appropriation (4,000 dollars) which had been granted to the United States Patent Office when in charge of it.

So rapidly were the treasures of the Museum increased by the gathered fruits of various government explorations and surveys, as well as by the voluntary contributions of the numerous and wide-spread tributaries of the Institution, that the policy was early adopted of freely distributing duplicate specimens to other institutions where they would be most appreciated and most usefully applied. And in this way the Smithsonian became a valuable centre of diffusion of the means of investigation in geology, mineralogy, botany, zoology and archæology. The clear foresight which announced that the Museum must very soon outgrow the entire capacity of the Smithsonian resources, has been most amply vindicated:§ and to-day a large Government Building is stored from basement to attic, with boxed up rarities of art and nature, sufficient more than twice to fill the Smithsonian

* *Smithsonian Report* for 1851, p. 227 (of Sen. ed.) p. 219, (of H. Rep. ed.)

† *Smithsonian Report*, for 1852, p. 253, (of Sen. ed.) p. 245, (of H. Rep. ed.)

‡ *Smithsonian Report* for 1853, p. 11, (of Sen. ed.) p. 9, (of H. Rep. ed.)

§ Although from the rapid growth of the national collection after it was transferred to the custody of the Smithsonian Institution, the annual appropriation of 4,000 dollars by Congress very soon became wholly insufficient to defray even one-half its necessary expenses, it was not till 1871 that the appropriation was raised to 10,000 dollars. In 1872, it was increased to 15,000 dollars, and in 1874, to 20,000 dollars.

halls and galleries, in addition to their present overflowing display. The strong desire of Henry to see established in Washington a National Museum on a scale worthy of our resources, and in which the existing overgrown collections might be so beneficially exhibited, he did not live to see gratified. That the realization of this wise and beneficent project is only a question of time, is little doubtful; and when established, its being and its benefits will in no small degree be due to him who first realizing its necessity, and most appreciating its importance, with unwearying perseverance for twenty-five years omitted no opportunity of urging upon members of Congress its importunate claims.

Meteorological Work.—In the conduct of what were appropriately called the "active operations" of the Institution—under the first section of the programme (in contradistinction to the local and statical objects of the second section), a rare energy and promptness were exhibited. The very first Report of the Secretary announced not only the acceptance and preparation for publication of an elaborate work on explorations by Messrs. Squier and Davis of "Ancient Monuments of the Mississippi Valley," but the commencement of official preparations "for instituting various lines of physical research. Among the subjects mentioned by way of example in the programme, for the application of the funds of the Institution, is terrestrial magnetism. . . . Another subject of research mentioned in the programme, and which has been urged upon the immediate attention of the Institution, is that of an extensive system of meteorological observations, particularly with reference to the phenomena of American storms. Of late years in our country more additions have been made to meteorology than to any other branch of physical science. Several important generalizations have been arrived at, and definite theories proposed, which now enable us to direct our attention with scientific precision to such points of observation as cannot fail to reward us with new and interesting results. It is proposed to organize a system of observations which shall extend as far as possible over the North American continent. . . . The present time appears to be peculiarly auspicious for commencing an enterprise of the proposed kind. The citizens of the United States are now scattered over every part of the southern and western portion of Northern America, and the extended lines of telegraph will furnish a ready means of warning the more northern and eastern observers to be on the watch for the first appearance of an advancing storm."*

* *Smithsonian Report* for 1847, pp. 146, 147, (of Sen. ed.), pp. 138, 139, (of H. Rep. ed.) Prof. Loomis (to whom among others "distinguished for their attainments in meteorology" letters inviting suggestions, had been addressed,) recommended that there should be at least one observing

An appropriation for the purpose having been made by the Regents, a large number of observers scattered over the United States and the Territories became voluntary correspondents of the Institution. Advantage was taken of the stations already established under the direction of the War, and of the Navy Departments, as well as of those provided for by a few of the States. The annual reports of the Secretary chronicled the extension and success of the system adopted; and in a few years between five and six hundred regular observers were engaged in its meteorological service. The favorite project of employing the telegraph for obtaining simultaneous results over a large area was at once organized; and in 1849, a system of telegraphic despatches was established, by which (a few years later) the information received in Washington at the Smithsonian Institution was daily plotted upon a large map of the United States by means of adjustable symbols. Espy's generalization that the principal storms and other atmospheric changes have an eastward movement,* was fully established by this rapidly gathered experience of the Institution; so that "it was often enabled to predict (sometimes a day or two in advance) the approach of any of the larger disturbances of the atmosphere."†

Eminently efficient as the enterprise approved itself, increasing experience served to demonstrate the increasing demands of the service; and it was seen that to prosecute the subject of meteorology over so large a territory, with the fulness necessary, would require a still larger force of observers, and a greater drain upon the resources of the Institution, than could well be spared from other objects; and as the great value of the system was fully recognized by the intelligent, the propriety of maintaining a meteorological bureau by the national support was early presented to the attention of Congress. This most important depart-

station within every hundred square miles of the United States; and he sagaciously pointed out that "When the magnetic telegraph [then an infant three years old] is extended from New York to New Orleans and St. Louis, it may be made subservient to the protection of our commerce." This interesting letter was published in full as "Appendix No. 2," to the *Report*. In 1848, a paper was read before the British Association by Mr. John Ball, "On rendering the Electric Telegraph subservient to Meteorological Research: in which the author suggested that simultaneous observations so collected, might reveal the direction and probable time of arrival of storms. (*Report Brit. Assoc.* Swansea. Aug. 1848. Abstracts, pp. 12, 13.)

* Franklin is said to have been the first who stated the general law, that the storms of our Southern States move off to the northeastward over the Middle and Eastern States.

† *Smithsonian Report* for 1864, p. 44. An interesting and instructive résumé of results accomplished within fifteen years was given in this *Report*, pp. 42–45: and continued in the succeeding *Report* for 1865, pp. 50–59.

ment of observation had been advanced by Henry to that position, in which a larger annual outlay than the entire income of the Institution was really required to give just efficiency to the system. In his Report for 1865, he remarked: "The present would appear to be a favorable time to urge upon Congress the importance of making provision for the reorganizing all the meteorological observations of the United States under one combined plan, in which the records should be sent to a central depot for reduction, discussion, and final publication. An appropriation of 50,000 dollars annually for this purpose would tend not only to advance the material interest of the country, but also to increase its reputation. . . . It is scarcely necessary at this day to dwell on the advantages which result from such systems of combined observations as those which the principal governments of Europe have established, and are now constantly extending."*

Five years later, in support of the proposition that the subject from its magnitude now appealed to the liberality of the nation, he briefly recapitulated the work accomplished by the limited means of the Institution. "The Smithsonian meteorological system was commenced in 1849, and has continued in operation until the present time. . . . It has done good service to the cause of meteorology; 1st in inaugurating the system which has been in operation upwards of twenty years: 2nd in the introduction of improved instruments after discussion and experiments: 3rd in preparing and publishing at its expense an extensive series of meteorological tables: 4th in reducing and discussing the meteorological material which could be obtained from all the records from the first settlement of the country till within a few years: 5th in being the first to show the practicability of telegraphic weather signals: 6th in publishing records and discussions made at its own expense, of the Arctic expeditions of Kane, Hayes, and McClintock: 7th in discussing and publishing a number of series of special records embracing periods of from twenty to fifty years in different sections of the United States,—of great interest in determining secular changes of the climate: 8th in the publication of a series of memoirs on various meteorological phenomena, embracing observations and discussions of storms, tornadoes, meteors, auroras, etc.: 9th in a diffusion of a knowledge of meteorology through its extensive unpublished correspondence and its printed circulars. It has done all in this line which its limited means would permit; and has urged upon Congress the establishment with adequate appropriation of funds, of a meteorological department under one comprehensive plan, 'in which the records should be sent to a central depot for reduction, discussion, and final publication.'"†

* *Smithsonian Report* for 1865, p. 57.
† *Smithsonian Report* for 1870, p. 43.

In 1870, a meteorological department was established by the Government under the Signal Office of the War Department, with enlarged facilities for systematic observation: and agreeably to the settled policy of the Institution, this important field of research was in 1872, abandoned in favor of the new organization.* Of the voluminous results of nearly a quarter of a century of systematic records over a wide geographical area which have been slowly digested and laboriously discussed, only a small portion has yet been published. The publication of the series when practicable, will yet prove an inestimable boon to meteorological theory.

Although our country can boast of many able meteorologists, who have greatly promoted our knowledge of the laws of atmospheric phenomena, it is safe to say that to no single worker in the field is our nation more indebted for the advancement of this branch of science to its present standing, than to Joseph Henry. Quite as much by his incitement and encouragement of others in such researches, as by his own exertions, does he merit this award. To him is undoubtedly due the most important step in the modern system of observation,—the installation of the telegraph in the service of meteorological signals and predictions.† While giving however his active supervision to the extensive system he had himself inaugurated, publishing many important reductions of particular features, as well as various circulars of detailed instructions to observers, of the desiderata to be obtained by those having the opportunities of arctic, oceanic, and southern explorations, directing the constant observations recorded at the Institution as an independent station, he made many personal investigations of allied subjects;—as of the Aurora, of Atmospheric electricity and Thunder-storms, of the supposed influence of the Moon on the weather,—and contributed a valuable series of Memoirs on Meteorology, embracing a wide range of physical exposition, to the successive Agricultural Reports of the Commissioner of Patents, during the years 1855, '56, '57,

* As an illustration of the popular favor in which this Signal service is held, it may be stated that the annual appropriation by Government for its support now exceeds not merely the entire Smithsonian income, but *sixteen times* that amount; or in fact its whole endowment.

† "However frequently the idea may have been suggested of utilizing our knowledge by the employment of the electric telegraph, it is to Professor Henry and his assistants in the Smithsonian Institution that the credit is due of having first actually realized this suggestion. . . . It will thus be seen that without material aid from the Government, but through the enlightened policy of the telegraph companies, the Smithsonian Institution *first in the world* organized a comprehensive system of telegraphic meteorology, and has thus given—first to Europe and Asia, and now to the United States, that most beneficent national application of modern science—the Storm Warnings." Article on "Weather Telegraphy" by Prof. Cleveland Abbe. (*Am. Jour. Sci.*, Aug. 1871, vol. ii. pp. 83, 85.)

'58, and 1859. Instructive articles on Magnetism and Meteorology were prepared in 1861 for the American Encyclopædia. And as an illustration of his continued interest in such studies, one of his latest published papers comprised a minute account of the effects of lightning in two thunder-storms; one occurring in the spring of last year (1877) at a Light-house in Key West, Florida, and the other occurring in the summer of last year at New London, Connecticut.*

Archæological Work.—One of the earliest subjects taken up for investigation by the Institution, was that of American Archæology; the attempt by extended explorations of the existing pre-historic relics, mounds, and monuments, of the aborigines of our country, to ascertain as far as possible their primitive industrial, social and intellectual character, and any evidences of their antiquity, or of their stages of development. The first publication of "Smithsonian Contributions" comprised in a good sized quarto volume an account of extensive examinations of the mounds and earthworks found over the broad valley of the Mississippi, with elaborate illustrations of the relics and results obtained: and this volume extensively circulated by gift and by sale, attracted a wide-spread attention and interest, and gave a remarkable stimulus to the further prosecution of such researches. "Whatever relates to the nature of man is interesting to the students of every branch of knowledge; and hence ethnology affords a common ground on which the cultivators of physical science, of natural history, of archæology, of language, of history, and of literature, can all harmoniously labor. Consequently no part of the operations of this Institution has been more generally popular than that which relates to this subject."†

Special explorations inaugurated by the Institution, have supplied it with important contributions to archæological information, and with the rich spoils of collected relics; which together with much material gathered from Arctic and from Southern regions, from Europe, from Asia, and from Africa, fill now a large museum hall 200 feet long and 50 feet wide, exclusively devoted to comparative Anthropology and Ethnology. In 1868, the Secretary reported that "during the past year greater effort has been made than ever before to collect specimens to illustrate the ethnology and archæology of the North American continent:" and he dwelt upon the importance of the subject as a study connecting all portions of the habitable earth, pointing out that "it embraces not only the natural history and peculiarities of the different races of men as they now exist upon the

* *Journal of the American Electrical Society*, 1878, vol. ii. pp. 37–43. The communication is dated Oct. 13, 1877; though not published till during his last illness.

† *Smithsonian Report* for 1860, p. 38.

globe, but also their affiliations, their changes in mental and moral development, and also the question of the geological epoch of the appearance of man upon the earth. . . . The ethnological specimens we have mentioned are not considered as mere curiosities collected to excite the wonder of the illiterate, but as contributions to the materials from which it will be practicable to reconstruct by analogy and strict deduction, the history of the past in its relation to the present."*

Two years later he reported: "The collection of objects to illustrate anthropology now in possession of the Institution is almost unsurpassed, especially in those which relate to the present Indians and the more ancient inhabitants of the American continent." Deprecating the frequent dissipation of small private collections of such objects at the death of their owners, he forcibly urges that "the only way in which they can become of real importance, is by making them part of a general collection, carefully preserved in some public institution, where in the course of the increasing light of science, they may be made to reveal truths beyond present anticipation."†

In his last Report—for 1877,—just published (and which he did not live to see in print), he says: "Anthropology, or what may be considered the natural history of man, is at present the most popular branch of science. It absorbs a large share of public attention, and many original investigators are assiduously devoted to it. Its object is to reconstruct as it were the past history of man, to determine his specific peculiarities and general tendencies. It has already established the fact that a remarkable similarity exists in the archæological instruments found in all parts of the world, with those in use among tribes still in a savage or barbarous condition. The conclusion is supported by evidence which can scarcely be doubted, that by thoroughly studying the manners and customs of savages and the instruments employed by them, we obtain a knowledge of the earliest history of nations which have attained the highest civilization. It is remarkable in how many cases, customs existing among highly civilized peoples are found to be survivals of ancient habits." He then argues from the significance thus developed of many trivial practices and unmeaning ceremonies handed down from immemorial time, the importance to a full comprehension of the customs of modern society, of a scientific study of the myths and usages of ancient peoples. "American anthropology," he remarks, "early occupied the attention of the Smithsonian Institution;" and alluding to its first published work, he says "from the time of the publication of this volume until the present, contributions of value have been made annually by the

* *Smithsonian Report* for 1868, pp. 26 and 33.
† *Smithsonian Report* for 1870, pp. 35, 36.

Institution to this branch of knowledge. . . . . The collection of the archæology and ethnology of *America*, in the National Museum, is the most extensive in the world: and in order to connect it permanently with the name of Smithson, it has been thought advisable to prepare and publish at the expense of the Smithsonian fund, an exhaustive work on American anthropology, in which the various classes of specimens shall be figured and described."* This great work still remains to be perfected.

Publications.—To attempt the recapitulation of the various branches of original research initiated or directly fostered by the Institution, would be to write its history. The range and variety of its active operations, and the value of their fruits, are in view of the limited income, and the collateral drains of less important objects exacted from it, something quite surprising. Scarcely a department of investigation has not received either directly or indirectly liberal and efficient assistance: and a host of physicists in the successful prosecution of their diverse labors, have attested their gratitude to the Institution, and no less to the ever sympathetic encouragement of its Director.

Over one hundred important original Memoirs, generally too elaborate to be published at length by any existing scientific society, issued in editions many times larger than the most liberal of any such society's issue, most of them now universally recognized as classical and original authorities on their respective topics, forming twenty-one large quarto volumes of "SMITHSONIAN CONTRIBUTIONS TO KNOWLEDGE," distributed over every portion of the civilized or colonized world, constitute a monument to the memory of the founder, James Smithson, such as never before was builded with the outlay of one hundred thousand pounds: and before which the popular Lyceums of our leading cities, with endowments averaging double this amount, pale into insignificance.

Such as these Lyceums with their local culture, admirable and invaluable in their way, but exerting no influence upon the progress of science, or outside of their own communities, and scarcely known beyond their cities' walls,—such was the type of institute which early legislators could alone imagine. Such as the "Smithsonian Institution" stands to-day,—such is the monument mainly constructed by the foresight, the wisdom, and the resolution of Henry.† All honor to the Regents, who with

* *Smithsonian Report* for 1877, pp. 22, 23. Circulars broadly distributed by the Institution, have served to give desired direction to popular attention and activity in this field of research; and the extent of co-operation is such as probably only the "Smithsonian" could have secured, unless by a vastly greater outlay.

† "It is not by its castellated building, nor the exhibition of the museum of the Government, that the Institution has achieved its present

an enlightenment so far in advance of the ruling intelligence of former days, and against the pressures of overwhelming preponderance of even educated popular sentiment, courageously adopted the programme of the Secretary and Director they had appointed; and who throughout his career, so wisely, nobly, and steadfastly upheld his policy and his purpose.

Fifteen octavo volumes of "Smithsonian Miscellaneous Collections" of a more technical character than the "Contributions," (including systematic and statistical compilations, scientific summaries, and valuable accessions of tabular "constants,") form in themselves an additional series; and represent a work of which any learned Society or Institution might well be proud. And thirty octavo volumes of annual Reports, rich with the scattered thoughts and hopes and wishes of the Director, form the official journal of his administration.

The Bibliography of Science.—Among the needful preparations for conducting original inquiry, none is more important than ready access and direction to the existing state of research in the particular field, or its allied districts. This information is scattered in the thousands of volumes which form the transactions of learned Societies; and its acquisition involves therefore in most cases a very laborious preliminary bibliographical research. To make this vast store of observation available to scientific students, by the directory of well arranged digests, would appear to fall peculiarly within the province of an Institution specially established for promoting the increase and diffusion of knowledge among men: and was early an object of particular interest to Henry. In his Report for 1851, he remarked: "One of the most important means of facilitating the use of libraries (particularly with reference to science,) is well digested indexes of subjects, not merely referring to volumes or books, but to memoirs, papers, and parts of scientific transactions and systematic works. As an example of this, I would refer to the admirably arranged and valuable catalogue of books relating to Natural Philosophy and the Mechanic Arts, by Dr. Young. This work comes down to 1807; and I know of no richer gift which could be bestowed upon the science of our own day, than the continuation of this catalogue to the present time. Every one who is desirous of enlarging the

reputation; nor by the collection and display of material objects of any kind, that it has vindicated the intelligence and good faith of the Government in the administration of the trust. It is by its explorations, its researches, its publications, its distribution of specimens, and its exchanges, constituting it an active living organization, that it has rendered itself favorably known in every part of the civilized world; has made contributions to almost every branch of science; and brought, more than ever before, into intimate and friendly relations, the Old and the New Worlds." (Memorial to Congress, by Chancellor S. P. Chase, and Secretary Joseph Henry. *Smithsonian Report*, for 1867, p. 114.)

bounds of human knowledge, should in justice to himself as well as to the public, be acquainted with what has previously been done in the same line; and this he will only be enabled to accomplish by the use of indexes of the kind above mentioned."*

At the time, and for years afterward, one-half of the Smithsonian income was diverted by the requirements of Congress to the local objects of the Lyceum: and the hopelessness of attempting a work—additional to that already mapped out, which would require the united labors of a large corps of well-trained and educated assistants for many years, and the subsequent devotion of the whole available income for many years following, to complete its publication, was fully realized. The project however was not abandoned: and in 1854, Henry conceived the plan of taking up the more limited department of *American* Scientific Bibliography; and by the persevering application of a fixed portion of the income annually for a succession of years, of finally producing a thorough subject-matter index, as well as an index of authors, for the entire range of American contributions to science from their earliest date. Inspired with this ambition, he sought to enlist the co-operation of the British Association for the Advancement of Science, in procuring with its large resources, a similar classified index for British and European scientific literature.

The favorable reception of this project, was officially announced to Henry by the Secretary of the Association, in the transmission of the following extract from the proceedings of that body for 1855. "A communication from Professor Henry of Washington having been read, containing a proposal for the publication of a catalogue of philosophical memoirs scattered throughout the Transactions of Societies in Europe and America, with the offer of co-operation on the part of the Smithsonian Institution, to the extent of preparing and publishing in accordance with the general plan which might be adopted by the British Association, a catalogue of all the American memoirs on physical science,—the Committee approve of the suggestion, and recommend that Mr. Cayley, Mr. Grant, and Professor Stokes be appointed a committee to consider the best system of arrangement, and to report thereon to the council."† The report of this committee dated 13th June, 1856, was presented to the succeeding Meeting of the British Association; in which they take occasion to say: "The Committee are desirous of expressing their sense of the great importance and increasing need of such a catalogue. . . The catalogue should not be restricted to memoirs in Transactions of Societies, but should comprise also memoirs in the Proceedings of Societies, in mathematical and scientific journals:"

* *Smithsonian Report* for 1851, p. 225 (of Sen. ed.), p. 217 (of H. Rep. ed.)
† *Report Brit. Assoc.* Glasgow, Sept. 1855, p. lxvi.

etc. . . "The catalogue should begin from the year 1800. There should be a catalogue according to the names of authors, and also a catalogue according to subjects."* The Committee comprising Fellows of the Royal Society of London finally succeeded in interesting that grave body in the undertaking: and the result was that greatly to Henry's satisfaction, the entire work was ultimately assumed by the Royal Society itself.

In the course of ten years that liberal Society aided by a large grant from the British Government gave to the world its half instalment of the great work, in its admirable "Catalogue of Scientific Papers" alphabetically classified by authors, in seven or eight large quarto volumes. In the Preface to this splendid monument of industry and liberality, stands the following history of its inception. "The present undertaking may be said to have originated in a communication from Dr. Joseph Henry, Secretary of the Smithsonian Institution, to the Meeting of the British Association at Glasgow in 1855, suggesting the formation of a catalogue of Philosophical memoirs: this suggestion was favorably reported on by a Committee of the Association in the following year. . . . In March, 1857, General Sabine, the Treasurer and Vice-President of the Royal Society, brought the matter before the President and Council of that body, and requested on the part of the British Association, the co-operation of the Royal Society in the project: whereupon a committee was appointed to take into further consideration the formation of such a Catalogue. . . . No further step was taken by the British Association or by the Royal Society in co-operation with that body: but the President and Council of the Royal Society acting on the recommendations contained in a Report of the Library Committee dated 7th January, 1858, resolved that the preparation of a Catalogue of scientific memoirs should be undertaken by the Royal Society independently, and at the Society's own charge."†

System of Exchanges.—For the diffusion of knowledge among men, one of the methods adopted by Henry from the very commencement of his administration was the organization of a system by which the scientific memoirs of Societies or of individuals from any portion of the United States, might be transmitted to foreign countries without expense to the senders: and by which

* *Report Brit. Assoc.* Cheltenham, Aug. 1856, pp. 463, 464.

† Preface to *Catalogue of Scientific Papers*, (1800–1863) vol. i. 1867, pp. iii., iv. The second and most important division of this great and invaluable work,—the classified Index to Subjects,—still remains to be accomplished. Had the plan adopted been made to include the scientific memoirs of the two preceding centuries, the value of the compilation would have been enhanced in a far greater proportion than the additional expenditure or the increase of bulk.

in like manner the similar publications of scientific work abroad might be received at the Smithsonian Institution, for distribution in this country. This privilege however is properly restricted to *bona fide* donations and exchanges of scientific memoirs;—all purchased publications being carefully excluded and left to find their legitimate channels of trade. By an international courtesy —creditable to the wisdom and intelligence of the civilized Powers,—such packages to and from the Institution are permitted to pass through all Custom-houses, free of duty; an invoice of authentication being forwarded in advance. When it is considered that this large work of collection and distribution (including the constant supply of the Institution's own publications, and the extensive returns therefor of journals, proceedings, and transactions, for its own library) requires the systematic records and accounts in suitable ledgers, with the accurate parcelling and labelling of packages, large and small, to every corner of the globe, it may well be conceived that no small amount of labor and expense is involved in these forwarding operations.* A recognition of the benefits conferred by this generous enterprise, is practically indicated by the rapid enlargement of the operations. The weight of matter sent abroad by the Institution at the end of the first decade, was 14 thousand pounds for the year 1857: the weight sent at the end of the second decade, was 22 thousand pounds for the year 1867: and the weight sent at the end of the third decade, was 99 thousand pounds for the last year 1877. This admirable system has been greatly encouraged and facilitated by the most praiseworthy liberality of the great lines of ocean steamers, and of the leading railway companies, in carrying the Smithsonian freight in many cases free of charge, or in other cases at greatly reduced rates: an appreciative tribute alike to the beneficent services and reputation of the Institution, and to the personal character and influence of its Director.†

* It may be stated that the number of foreign institutions and correspondents receiving the Smithsonian publications exceeds two thousand; whose localities embrace not only the principal cities of Europe (from Iceland to Turkey), of British America, Mexico, the West Indies, Central and South America, and of Australia, but also those of New Zealand, Honolulu in the Sandwich Islands, twelve cities in India, Shanghai in China, Tokio, Yedo, and Yokohama, in Japan, Batavia in Java, Manilla in the Philippine Islands, Alexandria and Cairo in Egypt, Algiers in northern Africa, Monrovia in Liberia, and Cape Town in southern Africa. The correspondents and recipients in the United States, are probably nearly as numerous.

† "The cost of this system would far exceed the means of the Institution, were it not for important aid received from various parties interested in facilitating international intercourse and the promotion of friendly relations between distant parts of the civilized world. The liberal aid extended by the steamship and other lines, mentioned in previous reports, in carrying the boxes of the Smithsonian exchanges free of charge, has been

"This part of the system of Smithsonian operations has everywhere received the commendation of those who have given it their attention or have participated in its benefits. The Institution is now the principal agent of scientific and literary communication between the old world and the new. . . . The importance of such a system with reference to the scientific character of our country, could scarcely be appreciated by those who are not familiar with the results which flow from an easy and certain intercommunication of this kind. Many of the most important contributions to science made in America have been unheard of in Europe, or have been so little known, or received so little attention, that they have been republished as new discoveries or claimed as the product of European research."* It would indeed be difficult to estimate rightly the benefit to science in the encouragement of its cultivators afforded by this fostering service. Few Societies are able to incur much expense in the distribution of their publications; and hence their circulation is necessarily very limited. The fructifying interchange of labors and results, dependent on their own resources, would be obstructed by the recurring expenses and delays of customs interventions, and by unconscionable exactions: and indeed without the Smithsonian mechanism, nine-tenths of the present scientific exchanges would be at once suppressed. Let it be hoped that so beneficent a system will not break down from the weight of its own inevitable growth.

Astronomical Telegraphy.—Analogous in principle to the system of exchange, is that adopted for the instantaneous trans-Atlantic communication of discoveries of a special order. In the year 1873, in the interests of astronomy (to which Henry was ever warmly devoted) he concluded "a very important arrangement between the Smithsonian Institution and the Atlantic cable Companies, by which is guaranteed the free transmission by telegraph between Europe and America of accounts of astronomical discoveries which for the purpose of co-operative observation require immediate announcement."† This admirable service to science, so creditable to the intelligence and the liberality of the Atlantic Telegraph Companies, embraces direct reciprocal communication between the Smithsonian Institution, and the foreign Observatories of Greenwich, Paris, Berlin, Vienna, and Pulkova. During the first year of its operation, four new planetoids were tele-

continued, and several other lines have been added to the number in the course of the year." (*Smithsonian Report* for 1867, p. 39.) Notwithstanding this unprecedented generosity, the exchange system has reached such proportions as to require for its maintenance one-fourth of the entire income from the Smithsonian fund.

* *Smithsonian Report* for 1853, p. 25, (of Senate ed.)

† *Smithsonian Report* for 1873, p. 32.

graphed from America, and seven telescopic comets from Europe to this country.

"Although the discovery of planets and comets will probably be the principal subject of the cable telegrams, yet it is not intended to restrict the transmission of intelligence solely to that class of observation. Any remarkable solar phenomenon presenting itself suddenly in Europe, observations of which may be practicable in America several hours after the sun has set to the European observer,—the sudden outburst of some variable star similar to that which appeared in *Corona borealis* in 1866,—unexpected showers of shooting-stars, etc., would be proper subjects for transmission by cable.

"The announcement of this arrangement has called forth the approbation of the astronomers of the world: and in regard to it we may quote the following passage from the fifty-fourth annual report of the Royal Astronomical Society of England: 'The great value of this concession on the part of the Atlantic telegraph and other Companies, cannot be too highly prized, and our science must certainly be the gainer by this disinterested act of liberality. Already planets discovered in America have been observed in Europe on the evening following the receipt of the telegram, or within two or three days of their discovery.'"*

Official Correspondence.—A vast amount of individual work having in view the diffusion of knowledge, has been performed by the correspondence of the Institution; which may be best described in the language of an extract from one of the early Reports. "There is one part of the Smithsonian operations that attracts no public attention, though it is producing important results in the way of diffusing knowledge, and is attended perhaps with more labor than any other part. I allude to the scientific correspondence of the Institution. Scarcely a day passes in which communications are not received from persons in different parts of the country, containing accounts of discoveries, which are referred to the Institution, or asking questions relative to some branch of knowledge. The rule was early adopted to give respectful attention to every letter received, and this has been faithfully adhered to from the beginning up to the present time. . . . Requests are frequently made for lists of apparatus, for information as to the best books for the study of special

* *Smithsonian Report* for 1873, p. 33. In 1876, a stellar outburst in the "Swan" observed by Dr. Schmidt of Athens, on the 24th of November, was announced. Less brilliant than the similar outburst which occurred in the northern "Crown" in May 1866, it continued to decline through the month of December, and at the close of the year, had dwindled from the third to the eighth magnitude. This may possibly be the same "temporary star"—seen in *Cygnus* in 1600, and again in 1670: and having therefore a period of variability of about 69 years.

subjects, for suggestions on the organization of local societies, etc. Applications are also made for information by persons abroad, relative to particular subjects respecting this country. When an immediate reply cannot be given to a question, the subject is referred by letter to some one of the Smithsonian co-laborers to whose line of duty it pertains, and the answer is transmitted to the inquirer, either under the name of the person who gives the information, or under that of the Institution, according to the circumstances of the case. . . . Many of those communications are of such a character, that at first sight it might seem best to treat them with silent neglect; but the rule has been adopted to state candidly and respectfully the objections to such propositions, and to endeavor to convince their authors that their ground is untenable. Though this course is in many cases attended with no beneficial results, still it is the only one which can be adopted with any hope of even partial good."*

The information given to scientific inquirers has been of an exceedingly varied and highly valuable character, not unfrequently involving a large amount of research from special experts; who have been accustomed cheerfully to bestow a degree of attention on difficult questions thus presented, which would have been accorded perhaps less ungrudgingly to others than to the universally honored Smithsonian Director. As to the pretensions and importunities of the unscientific,—such is the judgment pronounced after a quarter of a century of laborious experience with them:

"The most troublesome correspondents are persons of extensive reading, and in some cases of considerable literary acquirements, who in earlier life were not imbued with scientific methods, but who not without a certain degree of mental power, imagine that they have made great discoveries in the way of high generalizations. Their claims not being allowed, they rank themselves among the martyrs of science, against whom the scientific schools and the envy of the world have arrayed themselves. Indeed to such intensity does this feeling arise in certain persons, that on their special subjects they are really monomaniacs, although on others they may be not only entirely sane, but even evince abilities of a high order. . . . Two persons of this class have recently made a special journey to Washington, from distant parts of the country, to demand justice from the Institution in the way of recognition of their claims to discoveries in science of great importance to humanity; and each of them has made an appeal to his Representative in Congress to aid him in compelling the Institution to acknowledge the merits of his speculations. Providence vindicates in such cases the equality of its justice in giving to such persons an undue share of self-esteem

* *Smithsonian Report* for 1853, pp. 22, 23, (of Senate ed.)

and an exaltation of confidence in themselves, which in a great degree compensate for what they conceive to be the want of a just appreciation of the public. Unless however they are men of great benevolence of disposition, who can look with pity on what they deem the ignorance and prejudice of leaders of science, they are apt to indulge in a bitterness of denunciation which might be injurious to the reputation of the Institution, were their effects not neutralized by the extravagance of the assertions themselves."*

To the projectors and propellers of Paine electric engines, and Keely motors, eager for a marketable certificate from such an authority, Henry would calmly reply: "We may say that science has established the great fact—without the possibility of doubt, that what is called power, or that which produces changes in matter, cannot be created by man, but exists in nature in a state of activity or in a condition of neutralization; and furthermore that all the original forces connected with our globe, as a general rule have assumed a state of permanent equilibrium, and that the crust of the earth as a whole (with the exception of the comparatively exceedingly small proportion, consisting of organic matter such as coal, wood, etc.) is as it were a burnt slag, incapable of yielding power; and that all the motions and changes on its surface are due to actions from celestial space, principally from the sun. . . . All attempts to substitute electricity or magnetism for coal power must be unsuccessful, since these powers tend to an equilibrium from which they can only be disturbed by the application of another power, which is the equivalent of that which they can subsequently exhibit. They are however, with chemical attraction, etc., of great importance as intermediate agents in the application of the power of heat as derived from combustion. Science does not indicate in the slightest degree, the possibility of the discovery of a new primary power comparable with that of combustion as exhibited in the burning of coal. Whatever unknown powers may exist in nature capable of doing work, must be in a state of neutralization, otherwise they would manifest themselves spontaneously; and from this state of neutralization or equilibrium, they can be released only by the action of an extraneous power of equivalent energy; and we therefore do not hesitate to say that all declarations of the discovery of a new power which is to supersede the use of coal as a motive-power, have their origin in ignorance or deception, and frequently in both. A man of some ingenuity in combining mechanical elements, and having some indefinite scientific knowledge, imagines it possible to obtain a certain result by a given combination of principles, and by long brooding over this sub-

* *Smithsonian Report* for 1875, pp. 37, 38.

ject previous to experiment, at length convinces himself of the certainty of the anticipated result. Having thus deceived himself by his sophisms, he calls upon his neighbors to accept his conclusions as verified truths; and soon acquires the notoriety of having made a discovery which is to change the civilization of the world. The shadowy reputation which he has thus acquired, is too gratifying to his vanity to be at once relinquished by the announcement of his self-deception; and in preference he applies his ingenuity in devising means by which to continue the deception of his friends and supporters, long after he himself has been convinced of the fallacy of his first assumptions. In this way what was commenced in folly, generally ends in fraud."*

In looking back upon the struggles, conflicts, and obstructions of the past, it really seems quite marvellous that so much should have been accomplished, with so limited expenditure. These large results are partly due to the admirable method of the Secretary, his clear presage of effects, and his high power of systematic distribution and appliance; partly to the intelligent zeal and sympathetic energy of the able assistants whom he had associated with him almost from the organization of the institution; and partly to the personal magic of the man,—to the surprising amount of voluntary co-operation he was able to call forth in almost every direction, by the sheer force of his own earnest industry, and the contagious influence of his own devotion to the cause of scientific advancement.

An unwarranted Arraignment.—In 1855, while still with quiet determination maintaining the fundamental purpose of his Smithsonian administration against the pressure and opposition of powerful influences, (the discussion having been even carried to the halls of Congress,) Henry was made the subject of a most wanton and ungrateful public attack, in a pamphlet by Prof. Morse of Telegraph fame, impugning not only his scientific reputation, but for the first time—the truthfulness of his testimony given in certain telegraph suits some half a dozen years previously, in reluctant obedience to legal summons.† This testimony thus exacted, of course failed to sustain the complainant's exorbitant claims to the absolute invention and ownership of all possible forms of the electro-magnetic telegraph,

* *Smithsonian Report* for 1875, pp. 39, 40.

† The Hon. S. P. Chase, while Governor of Ohio, (subsequently Chief Justice of the Supreme Court of the United States,) in a letter dated Columbus, Nov. 26, 1856, after reciting his professional connection with the litigations of 1849, says: "I remember very well that you were unwilling to be involved in the controversy even as a witness, and that you only submitted to be examined in compliance with the requirements of law. Not one of your statements was volunteered."

and correspondingly failed to satisfy the cupidity of the actual prosecutors: and in this remarkable accusation first published in 1855, could readily be discerned the mercenary inspiration of interested capitalists and assignees—anxious only to stretch the monopoly to its extremest grasp. To Prof. Morse himself in his early efforts, Henry had generously rendered every encouragement and assistance; and in his later successes had as freely extended his congratulations and his testimonials of the practical merits of his invention.*

To descend to a personal controversy with Mr. Morse, was utterly repugnant to Henry's feelings: to permit his serious impeachment to stand untraversed, appeared scarcely less objectionable. With a calm and self-respecting dignity, Henry simply presented the published arraignment to the Board of Regents, for their consideration and action, with a communication dated March 16, 1857, in the following terms:

"Gentlemen: In the discharge of the important and responsible duties which devolve upon me as Secretary of the Smithsonian Institution, I have found myself exposed like other men in public positions to unprovoked attack and injurious misrepresentation. Many instances of this it may be remembered, occurred about two years ago, during the discussions relative to the organic policy of the Institution: but though very unjust, they were suffered to pass unnoticed; and generally made I presume no lasting impression on the public mind. During the same controversy however, there was one attack made upon me of such a nature, so elaborately prepared and widely circulated by my opponents, that though I have not yet publicly noticed it, I have from the first thought it my duty not to allow it to go unanswered. I allude to an article from the pen of Prof. S. F. B. Morse, the celebrated inventor of the American electro-magnetic telegraph. In this, not my scientific reputation merely, but my moral character was pointedly assailed: indeed nothing less was attempted than to prove that in the testimony which I had given in a case where I was at most but a reluctant witness, I had consciously and wilfully deviated from the truth, and this too from unworthy and dishonorable motives. Such a charge, coming from such a quarter, appeared to me then as it appears now, of too grave a character and too serious a consequence, to be withheld from the notice of the Board of

* "It was my wish in every statement to render Mr. Morse full and scrupulous justice. While I was constrained therefore to state that he had made no discoveries in science, I distinctly declared that he was entitled to the merit of combining and applying the discoveries of others in the invention of the best practical form of the magnetic telegraph. My testimony tended to establish the fact that though not entitled to the exclusive use of the electro-magnet for telegraphic purposes, he was entitled to his particular machine, register, alphabet, etc. This however did not meet the full requirements of Mr. Morse's comprehensive claim."

Regents: . . . and I now embrace the first opportunity of bringing the subject officially to your notice, and asking from you an investigation into the justice of the charges alleged against me. And this I do most earnestly, with the desire that when we shall all have passed from this stage of being, no imputation of having attempted to evade in silence so grave a charge shall rest on *me*,—nor on *you*, of having continued to devolve upon me duties of the highest responsibility, after that was known to some of you individually, which if true—should render me entirely unworthy of your confidence. Duty to the Board of Regents, as well as regard to my own memory, to my family, and to the truth of history, demands that I should lay this matter before you, and place in your hands the documents necessary to establish the veracity of my testimony so falsely impeached, and the integrity of my motives so wantonly assailed."*

Professor Felton, President of Harvard University, Chairman of the select Committee appointed by the Regents to investigate the charge, after a careful examination of all the documentary evidence, submitted a full report, from which it is only necessary to make the following extracts with reference to the Morse pamphlet: "The first thing which strikes the reader of this article is that its title is a misnomer.† It is simply an assault upon Professor Henry; an attempt to disparage his character; to deprive him of his honors as a scientific discoverer; to impeach his credibility as a witness and his integrity as a man. It is a disingenuous piece of sophistical argument, such as an unscrupulous advocate might employ to pervert the truth, misrepresent the facts, and misinterpret the language in which the facts belonging to the other side of the case are stated. . . . Your committee come unhesitatingly to the conclusion that Mr. Morse has failed to substantiate any one of the charges he has made against Professor Henry, although the burden of proof lay upon him; and that all the evidence—including the unbiased admissions of Mr. Morse himself—is on the other side. Mr. Morse's charges not only remain unproved, but they are positively disproved." And the committee submitted resolutions of condemnation on the one side, and of respect and confidence on the other, which were unanimously adopted by the Regents, and made a part of the permanent record.‡

* *Smithsonian Report* for 1857, pp. 85, 86.

† "A Defence against the injurious deductions drawn from the Deposition of Professor Joseph Henry, by Samuel F. B. Morse."

‡ *Smithsonian Report* for 1857, pp. 89–98. When Prof. Morse in his letter to Prof. Sears C. Walker, dated Washington, Jan. 1st, 1848, wrote thus: "To Prof. Henry is unquestionably due the honor of the discovery of a fact in science which proves the practicability of exciting magnetism through a long coil or at a distance, either to deflect a needle or magnetize soft iron;" and when again some six years later, the same Prof. Morse in his pamphlet dated Locust Grove, New York, December, 1853,

Scientific Observatories.—One of the objects very dear to Henry's heart, was the establishment of a physical observatory (with a physical laboratory in connection) for the systematic observation and record of important points in celestial and terrestrial physics. For the proper maintenance of such an establishment, he thought an income as large as that of the Smithson fund, would not be too large: and on two different occasions he endeavored to enlist the interest of wealthy and public-spirited citizens in such an enterprise. One of these was Mr. McCormick of Virginia; and a letter on the subject was afterward printed (without its address) in the Report for 1870.* The other was Mr. Lick of California: who after some hesitation, decided in favor of an astronomical observatory. Another allied object of great interest to Henry—and one requiring as large an endowment—was a well-equipped chemical laboratory, in which—under judicious restrictions—those really engaged in original researches, should have liberal facilities of appliances and needed materials, furnished them. He considered that an important part of the work to be accomplished by a physical and chemical laboratory, would be the determination and tabulation of "The Constants of Nature and Art" with a much wider range of subject, and on a scale of much greater completeness and accuracy, than had heretofore been attempted: and thus might be realized the great work or works of reference, suggested by Babbage as a scientific *desideratum.*† Had the Smithsonian fund been twice as large as it is, both these great enterprises for the increase of knowledge, would undoubtedly have been successfully inaugurated by Henry.

Loss by Fire.—Early in the year 1865, (on the 24th day of January,) the central portion of the Smithsonian Building suffered from a disastrous fire, the effects of which were aggravated by the extreme severity of the winter cold, which greatly obstructed the efficiency of the engines brought into action.‡ "The progress of the fire was so rapid, that but few of the contents of the upper rooms could be removed before the roof fell in. The conflagration

wrote: "I shall show that I am not indebted to him for any discovery in science, bearing on the Telegraph:" (p. 9,)—it may be confidently assumed from his known but singular unfamiliarity with scientific literature, that equally in either case he but echoed the promptings of others, and equally in either case in entire ignorance of the real facts. To his dying day, he probably sincerely failed to apprehend the nature of his indebtedness to Henry.

* *Smithsonian Report* for 1870, pp. 141-144.

† *Smithsonian Report* for 1856, pp. 289-302.

‡ The accident resulted from the carelessness of some workmen in the upper picture gallery, who in temporarily setting up a stove, inserted the pipe through a wall-lining into a furring space (supposing it a flue), but which conducted directly under the rafters of the roof.

was only stayed by the incombustible materials of the main building:" the flooring of the upper story, forming an iron and brick vaulting over the lower or principal story. Neither wing of the building was reached by the fire; and the valuable Library (not then transferred to the Capitol), and the Museum, fortunately escaped without injury. The Stanley collection of Indian portraits, comprising about 200 paintings, and estimated as worth 20,000 dollars, was entirely destroyed. A fine full-sized copy in Carrara marble, by John Gott, of the antique statue known as "The Dying Gladiator," was crumbled into a formless mass of stone.

The Secretary's office unfortunately fell within the range of the flames. "The most irreparable loss was that of the records, consisting of the official, scientific, and miscellaneous correspondence; embracing 35,000 pages of copied letters which had been sent, (at least 30,000 of which were the composition of the Secretary,) and 50,000 pages of letters received by the Institution; the receipts for publications and specimens; reports on various subjects which have been referred to the Institution; the records of experiments instituted by the Secretary for the Government; four manuscripts of original investigations, [memoirs by collaborators,] which had been adopted by the Institution for publication; a large number of papers and scientific notes of the Secretary; a series of diaries, memorandum and account books."* This truly "irreparable loss" of the original notes of many series of experiments by Henry, of varied character, running back for thirty years, kept for the purpose of reduction and discussion, or further extension (as leisure might permit) and of which but few had been published even by results,—was borne by their author with his characteristic equanimity; and was very rarely alluded to by him, unless when in answer to inquiries respecting particular points of his researches, he was compelled to excuse the absence of precise data.

The Lecture Room—a model of its class—entirely burned out by the fire, was not reconstructed: but the space it occupied on the upper floor, was with the adjacent rooms (used as the apparatus room, and the art gallery) thrown into one large hall, 200 feet long,—at present occupied as the ethnological museum. Advantage was taken of the hazard demonstrated by the fire, to induce Congress in the following year to transfer the custody of the Smithsonian collection of scientific works to the National Library: and the propriety of this change was thus defended. "The east wing of the Smithsonian building, in which the books were deposited is not fire-proof, and is liable to destruction by accident or the torch of the incendiary, while the rooms of the Capitol are of incombustible materials. This wing was more-

* *Smithsonian Report* for 1865, p. 18.

over filled to overflowing; and a more extended and secure depository could not be obtained, except by another large draught on the accumulated funds intended to form part of the permanent capital."*

Second Visit to Europe.—At a meeting of the Board of Regents, held February 3rd, 1870, "General Delafield in behalf of the Executive Committee, stated that they deemed it highly important for the interests of the Institution in the promotion of science, and due to the Secretary for his long and devoted services, that he should visit Europe to consult with the savans and societies of Great Britain and the continent; and he therefore hoped that a leave of absence would be granted to Professor Henry for several months, and an allowance be made for his expenses. On motion of Dr. Maclean it was unanimously Resolved, That Professor Henry, Secretary of the Institution, be authorized to visit Europe in behalf of the interests of the Smithsonian Institution, and that he be granted from three to six months leave of absence, and two thousand dollars for travelling expenses for this purpose."†

It is not necessary here to recount the particulars of this second visit of Henry to Europe, more fully than in the brief account given by him in his annual Report. "Before closing this report, it is proper that I should refer to a resolution adopted by your honorable Board at its last session, granting me leave of absence to visit Europe to confer with savans and societies relative to the Institution, and making provision for the payment of my expenses. The presentation of this proposition was entirely without my knowledge, but I need scarcely say that its unanimous adoption was highly gratifying to my feelings; and that I availed myself of the privilege it offered with a grateful appreciation of the kindness intended. I sailed from New York on the 1st of June, returning after an absence of four and a half months, much improved in health, and with impressions as to science and education in the Old World, which may be of value in directing the affairs of the Institution. Although limited as to time, and my plans interfered with somewhat by the war, I visited England, Ireland, Scotland, Belgium, parts of Germany and France. But deferring for the present an account of my travels, and the observations connected with them, I will merely state that as your representative, I was everywhere kindly received, and was highly gratified with the commendations bestowed on the character and operations of the Institution intrusted to your care."‡

* *Smithsonian Report* for 1866, p. 14.
† *Smithsonian Report* for 1869, p. 89.
‡ *Smithsonian Report* for 1870, p. 45.

Service on the Light-house Board.—While the whole high bent of Henry's mind was rather toward abstract than utilitarian research, there was no well devised system of practical benefit for man, that did not command his earnest sympathy or enlist his active co-operation:—no labor in such co-operation from which he shrank, if he felt that without the sacrifice of other duties, he could make such labor useful. On the establishment of the Light-house Board, in 1852, Henry was appointed as one of its members; and although his valuable time was already fully occupied, he consented to serve on the Board, in the hope of aiding to benefit the interests of navigation. To the requirements of his new position, he brought his accustomed energy, skill, and eminently practical judgment; and soon made his influence felt throughout the Light-house service.

When the steadily advancing cost of whale oil made it necessary to seek for some more economical illuminant, he attacked the problem with his habit of scientific method. Colza oil or rape-seed oil had been used in France with some success; and efforts were made to introduce its culture and production in this country. Lard-oil had been tested by Professor J. H. Alexander of Baltimore, and pronounced by him of very inferior value as an illuminant. For accuracy of determination, Henry caused to be prepared at the Light-house Depot on Staten Island, a long dark fire-proof chamber, and had it painted black on all its interior surfaces for the purpose of photometric observations. In ordinary lamps, the colza oil was found to be nearly equal to whale oil in illuminating power, and lard-oil inferior to it. Petroleum or mineral oil was also tried; but its quality was at that time too variable, and its use was found to be too dangerous. Experiment showed that lard-oil had a greater specific gravity than sperm oil, a less capillarity or ascensional attraction in a wick, and a less perfect fluidity. The conditions were varied; and it was found that with elevation of temperature, the fluidity, and the capillarity, of the lard-oil increased more rapidly than those of the sperm oil, until at about 250° F. the former surpassed the latter in these qualities. With these results, it became important to compare the oils in large lamps, such as were actually required for the lanterns of light-houses. The heat evolved by the large sized Argand burners, would seem peculiarly to favor the lard-oil: a few trials, with a proper adaptation of the lamps, established its supremacy; and conclusively demonstrated—contrary to all the laboratory trials of former experimenters, that for the purpose desired, this contemned article was for equal quantities a more brilliant illuminant than mineral kerosene-oil, or vegetable colza-oil, or animal sperm-oil, while its market price was only about one-fourth that of the latter. Against all the opposition of interested dealers, and prejudiced keepers, the lard-

oil was at once introduced into actual use in the years 1865 and 1866, in all the light-houses of the United States; with an economy of at least one dollar on every gallon of the hundred thousand in annual use; that is of 100,000 dollars per annum.

During the progress of these useful labors, no less important investigations were commenced, on the most efficient forms of apparatus for acoustic signalling, as the substitutes for light signals during the prevalence of sea-board fogs. "Among the impediments to navigation, none are perhaps more to be dreaded than those which arise from fogs. . . . The only means at present known for obviating the difficulty, is that of employing powerful sounding instruments which may be heard at a sufficient distance through the fog, to give timely warning of impending danger."*

Gun signals were early abandoned, as inefficient, dangerous, and expensive: inefficient, because of both "the length of the intervals between the successive explosions, and the brief duration of the sound, which renders it difficult to determine with accuracy its direction." Innumerable projects eagerly pressed upon the Board by visionary inventors (some of them being rattles, gongs, or organ pipes operated by manual cranks, many of them being varieties of automatic horn or whistle operated by the winds or the waves) were impartially tested, and uniformly rejected as wholly insufficient: very few of their projectors having the slightest practical idea of the requirements of the service. Experiments on steam-whistles of large size and on horns with vibrating steel tongues or reeds, sounded by steam-power, or by hot-air engines, varied and continued for several years under wide changes of conditions, finally determined their most efficient size and character.†

In 1867, comparative trials were made at Sandy Hook (on the Jersey shore, at the entrance to Raritan Bay, and to New York Bay,) with three powerful instruments; a large steam whistle whose cup was 8 inches in diameter, and made adjustable in pitch; a large reed trumpet 17 feet long and 38 inches in diameter at its flaring mouth, whose steel tongue was 10 inches long, 2¾ inches wide, and half an inch thick at its smaller vibrating end, and was blown by a hot air engine; and lastly a large

* *Report of L. H. Board* for 1874, p. 83.

† An enterprising inventor had secured a patent for a metallic compound or alloy for steam-whistles, especially adapted to increase greatly their power as fog-signals. In vain was he assured that his "improvement" was a fallacy; that the cylindrical cup of the whistle was not a bell, but only a resonant chamber; and that its material was comparatively unimportant. He was only with difficulty convinced, when Henry had his whistle formally tested, with a stout cord wound tightly around its cylindrical surface: when its tone under steam escape was proved to be as full, as loud, and as penetrating, as with the cord removed.

siren horn operated by steam at different pressures, the aerial vibration being produced by the intermittence of a revolving grating or disk valve in the small end of the horn, driven at high velocities by the steam engine, and its pitch regulated by the adjustable speed of the revolving disk. The trumpet or fog-horn was provided with a series of replaceable steel tongues of different sizes, and the siren was driven at five different pitches of from 250 to 700 impulses per second, and at steam pressures varying from 100 pounds to 20 pounds per square inch. For the purpose of accurate estimation, within short distances, a phonometer or "artificial ear" was employed, having at its smaller upturned end a horizontal drum of stretched membrane, sprinkled with sand, after the plan devised by Sondhauss. Trumpets of the same size, were made of different materials, as of brass, iron, and wood; but these differences were found to exercise little or no influence on the intensity or penetration of the sound. Trumpets were also made of different shapes, straight and curved, and square as well as round, with equal lengths and equal areas of cross section; from whose trials it appeared that the conical form gave nearly double the distance of action on the sand of the "artificial ear," that was given by the pyramidal form. Such investigations—varied and long-continued, serve to show the conscientious earnestness with which Henry sought to give the highest efficiency to the expedients available for the protection of life and property along our extended sea-coast.

The steam-whistle was found to be less powerful than the trumpet, with the same expenditures of fuel. Steam whistles were afterwards tried of 10 inches, 12 inches, and 18 inches in diameter. The largest size was not found to give results proportioned to its increased consumption; and the 10 or 12 inch size was regarded as practically the most efficient. The siren was found to be the most powerful and penetrating of the instruments tested, as it admitted more advantageously the application of a higher steam expenditure. The best result with this instrument was attained with a pressure of from 60 to 80 pounds, and at a pitch between 350 and 400 vibrations per second. Under favorable conditions, this instrument frequently made itself heard at a distance of fifteen and twenty miles. Henry's large experience with the occasional aerial impediments to sound propagation,* and his strong sense of the vital importance of having fog-signals recognized at a distance, under the most adverse conditions, led him to favor the introduction of the most powerful sounders attainable, without absolutely limiting the decision to their relative economy. Hence he was the first to devise improvements in the siren, and to press its adoption at important or

* An abstract of his elaborate and invaluable researches on some abnormal phenomena of Sound—the crowning labor of his life, must be reserved for a concluding section.

dangerous stations, notwithstanding its higher consumption of steam or heat power.*

Partly under the stimulus given to the sale of lard-oil by the striking proofs of its excellence as an illuminant under favorable conditions, furnished by Henry, this article slowly advanced in price; though probably not to an extent of more than a fourth part additional cost. Henry's energies again were called into requisition to devise a remedy. Neither gas, nor electricity, the favorite means of numerous projectors and advisers, appeared justified, on the score of economy.† A new series of elaborate experiments was undertaken to determine whether mineral oil (so abundant as to be easily procurable at one-third the cost of lard oil) could not be made available. The great improvements introduced into its preparation in later years by high distillation, seemed to justify the attempt. Not only was a laborious inquiry into the best conditions of combustion, by precise photometric measurement required, but for the security of the service, equally laborious examinations into the best practicable methods of testing, of handling, and of storing this material.‡ To secure a

* Major G. H. Elliott, commissioned by the U. S. Light-house Board to make a tour of inspection of European Light-house establishments in 1873, in his Report published by the Senate in 1874. says of the British and French systems, "I saw many details of construction and administration which we can adopt to advantage, while there are many in which we excel. Our shore fog-signals particularly, are vastly superior both in number and power." (*Report on European Light-houses*, p. 12.) "To the careful and laborious investigations and experiments of the distinguished Chairman of the Light-house Board, prolonged through a series of years, and prosecuted under a great variety of conditions, is largely to be attributed the acknowledged superiority of our fog-signal service." (*Journal of Franklin Institute*, Jan. 1876, vol. lxxi. p. 43.)

† *Report of L. H. Board* for 1874, p. 11. No agency (for whatever purpose) has proved so enticing to the half-informed as *electricity*. For years past scarcely a month has elapsed without some new form of patent electric-light, or some marvellous application of electric-lights, being pertinaciously urged by sanguine "reformers" upon the Light-house Board for adoption; some of these ideal schemes being the mounting of electric-lights on buoys, or on the masts of light-ships, or their suspension from moored balloons. Many eminently original minds have earnestly desired to obtain contracts for supplying all the Light-houses with oxy-hydrogen lime lights. In a fog, the most powerful electric-light is as useless as the cheapest kerosene lamp.

‡ "It has been established that the ordinary fire-test is insufficient as usually applied, and that an explosive mixture may be formed by confining the vapors given off at a temperature in some cases twenty degrees lower than that certified to by the public inspector. That this inquiry is of great practical importance to the Light-house system, must be evident when we reflect that means must be devised for testing the oil offered for acceptance in accordance with contracts; for storing it; for transporting it to light-house stations; for preserving it in butts at the stations; and for the instruction of the keepers in its daily use." (*Report L. H. B.* 1877, p. 5.)

proper oxygenation in burning, a modification of the lamp was required. "It was soon apparent that the use of mineral oil would necessitate a change of lamps, and attention is now directed to the perfection of one which will produce the best results from this illuminant. It is thought that the lamps now used with lard-oil can be converted at no great expense and successfully used with mineral oil. Our experiments have shown that this oil can be more readily used in the smaller lamps; and it is proposed as soon as suitable ones can be prepared, to put it into use at such stations of the fifth and sixth order, as may be thought expedient; when if it be found satisfactory, an attempt will be made to substitute it for lard-oil in lamps of the higher orders."* "This change is proposed entirely with reference to economy; for it has been found by repeated experiment, that while a somewhat superior light may be obtained from a small lamp charged with kerosene, a larger lamp charged with lard-oil affords the greater illuminating power. So great is this difference in lamps of the first order with five wicks, that the rates of light from kerosene and lard, are as three to four respectively. Since the safety of the keeper and the continuity of the light are essential elements in the choice of an illuminant, a thorough acquaintance with the nature of the substance is essentially necessary. With a view therefore to the introduction of kerosene, a series of experiments have been made during the last two years on the different varieties of this material found in the market."†

In 1871, on the resignation of Admiral Shubrick, Henry was chosen as the Chairman of the Light-house Board; and his energetic labors in behalf of the service, fully vindicated the wisdom of the choice. Punctual in his attendance on the weekly meetings of the Board, he inspired others with a portion of his own zealous devotion. Nor did he fail to urge upon the Government the constant need and responsibility of maintaining an efficient establishment. He emphatically declared that "the character of the aids which any nation furnishes the mariner in approaching and leaving its shores, marks in a conspicuous degree its advancement in civilization. Whatever tends to facilitate navigation or to lessen its dangers, serves to increase commerce; and hence is of importance not only to the dwellers on the seaboard, but to the inhabitants of every part of the country. . . . Therefore it is of the first importance that the signals, whether of light or sound, which indicate the direction of the course, and the beacons which mark the channel, shall be of the most improved character, and that they be under the charge of intelligent, efficient, and

* *Report L. H. Board,* 1875, p. 6.
† *Report L. H. Board,* 1877, p. 4.

trustworthy attendants."* And rising to a higher argument, he pointed out that "it is not alone in its economical aspect that a light-house system is to be regarded: it is a life preserving establishment founded on the principles of Christian benevolence, of which none can so well appreciate the importance as he who after having been exposed to the perils of the ocean—it may be for months—finds himself approaching in the darkness of night a lee shore. But it is not enough to erect towers, and establish other signals: they must be maintained in an efficient state with uninterrupted constancy."†

A formal report made to the Hon. Secretary of the Treasury by the Naval Secretary of the Light-house Board, dated May 21st, 1878, (very shortly after Henry's death,) simply detailing for information, the character of his gratuitous services to the Light-house establishment during a quarter of a century, (and not intended for the public,) takes the inevitable form of eulogy. A portion of it is here quoted.

"As chairman of this committee, Professor Henry acted as the scientific adviser of the Board. But in addition it was his duty to conduct the experiments made by the Board, not only in the matter of original investigation and testing of the material used, but in examining and reporting on the models, plans, and theories presented by others to the Board. The value of the services he rendered in this position is simply inestimable. He prepared the formula for testing our oils; he conducted the series of experiments resulting in the substitution of lard-oil for sperm oil, which effected an immense saving in cost; and he also conducted the experiments which have resulted in making it possible to substitute mineral oil for lard-oil, when another economy will be made. His original investigation into the laws of sound have resulted in giving us a fog-signal service conceded to be the best in the world. His examinations into the action of electricity, has enabled the Board to almost completely protect its stations from the effect of lightning. The result of his patient, continuous, practical experimentation is visible everywhere in the

* *Report of L. H. Board*, 1873, pp. 3, 4. The coast line of the United States is far more extended than that of any other nation on the globe. "The magnitude of the Light-house system of the United States may be inferred from the following facts: from the St. Croix River on the boundary of Maine, to the mouth of the Rio Grande in the Gulf of Mexico, includes a distance of over 5,000 miles; on the Pacific coast, a length of about 1,500 miles; on the great northern Lakes, about 3,000 miles; and on inland rivers about 700 miles; making a total of more than 10,000 miles. Nearly every square foot of the margin of the sea throughout the whole extent of 5,000 miles along the Atlantic and Gulf coast is more or less illuminated by Light-house rays; the mariner rarely losing sight of one light until he has gained another." (p. 4, of same Report.)

† *Report L. H. B.* 1874, p. 5.

service. No subject was too vast for him to undertake; none too small for him to overlook. And while he has brought into the establishment so many practical applications of science, he has done almost as much service by keeping out what presented by others seemed plausible, but which on examination proved impracticable.

"Every theory, plan, or machine, which was pressed on the Board, as for the interests of commerce and navigation, was referred to the committee on experiments, when it was examined by its Chairman, and was formally reported upon. If it had no practical value, the report on record simply stated the inexpediency of its adoption: but the Professor often verbally pointed out to the presenter its fallacy; and sent him away—if not satisfied—at least feeling that he had been well treated. He thus prevented not only the adoption of impracticable plans, but avoided the enmity of their inventors.

"Professor Henry made many valuable reports, containing the results of his elaborate experiments into matters which were formally referred to him, which are spread on the records of the Board; and the reports were drawn in such form that his suggestions were capable of and received practical application. But in addition to this, he was constantly extending his scientific researches for the benefit of the service in all directions. His summer vacations were as a rule passed in experimentation at the laboratory of the Establishment at Staten Island, on its steamers, or at its light-stations, pushing his inquiries to their last results. To experimentation in the interests of this service, Professor Henry seemed to give his whole heart. It appeared as if he never lost sight of the needs of the Establishment, and as if he never neglected an opportunity to advance its interests. In addition to his other duties, Professor Henry presided as Chairman of the Light-house Board for the last seven years at its weekly meetings, when he did much to infuse into the different members of the Board, his own spirit of labor for, and devotion to its interests."*

Services to the National Government.—The value of Henry's services to the various Executive Departments of our Government, faithfully and unostentatiously performed through a long series of years and a succession of Presidential Administrations, cannot be estimated, as its history can never be written. Whatever material for it existed in the form of abstracts of inquiries, trials, and reports, prior to 1865, unfortunately perished in the

* *Executive Documents*, No. 94, Forty-fifth Congress, 2d Session, Senate, pp. 2, 3. It is gratifying to know that on the presentation of his report and recommendation to Congress, by the high-minded Secretary of the Treasury, a moderate appropriation in slight recognition of Henry's "inestimable" services was at once passed for the benefit of his bereaved family.

dre of that year. Whenever in any important case a scientific adviser could be useful to the proper conduct of a Bureau, Henry's reputation generally pointed him out as the most suitable expert and arbiter. On the outbreak of the great civil war, the number of such references was naturally very considerably increased. The Departments of War, of the Navy, and of the Treasury, were besieged by projectors with every imaginable and impossible scheme for saving the country, and demolishing the enemy. Torpedo balloons, electric-light balloons, wonderful compounds destined to supersede gunpowder, and revolutionize the art of war; cheap methods for the manufacture of Government bonds and paper-money, multitudinous expedients for the prevention of counterfeiting, by devices in the engraving, by secret markings, by anti-photographic inks, by peculiar textures of paper,—applicable to coupons, to circulating notes, to revenue stamps,—each warranted to be infallible,—such were among the agencies by which patriotic patentees and adroit adventurers were willing to serve their country and to reap their reward by the moderate royalty or percentage due to the magnificence of the public benefit. Such were among the unenviable tasks of examination and adjudication accepted by Henry, only from an intrepid sense of duty.

"The course which has been pursued of rendering the Government in its late trials, every aid which could be supplied by scientific research, has been warmly approved. As most persons are probably entirely ignorant of the services really rendered to the Government by the Institution, I may here state the fact that a large share of my time, (all indeed which could be spared from official duties,) has been devoted for the last four years to investigations required by the public exigencies. Within this period, several hundred reports, requiring many experiments, and pertaining either to proposals purporting to be of high national importance, or relating to the quality of the multifarious articles offered in fulfillment of legal contracts, have been rendered. The opinions advanced in many of these reports, not only cost much valuable time, but also involved grave responsibilities. While on the one hand the rejection of a proposition would be in contravention to the high importance claimed for it by its author, on the other the approval of it would perhaps incur the risk of the fruitless expenditures of a large amount of public money. It is not necessary, I trust, to say that the labor thus rendered was entirely gratuitous, or that in the judgment pronounced in any case, no regard was paid to the interested solicitations or personal influence of the parties concerned: on the contrary it has in some instances resulted from the examination of materials sold to the Government, that attempted fraud has been exposed, and the baffled speculator received his due reward in condemnation and punishment. These facts it is thought will be deemed a

sufficient answer to those who have seemed disposed to reproach the Institution with the want of a more popular demonstration —but of a really far less useful or efficient aid in the support of the Government."*

In the performance of these troublesome and often disagreeable labors, conducted with the single aim necessitated by all his scientific habits and instincts, it of course resulted that a great majority of his judgments and recommendations were decidedly adverse to the hopes and wishes of the aspirants to fame and fortune. Having once satisfied himself of the frivolity or the chicanery of an article or project, his decision was inflexible; and although importunate appeals to the Department Secretary, abetted by a prostituted political or other influence, in one or two instances succeeded in fastening for a time upon the public Treasury a worthless or a noxious leech, the vast number of such, excluded from experimental imbibitions by Henry's critical supervision, must have been a protection to the public interests quite beyond the reach of estimation: while the supplies of honest contractors awarded their just commendation, and the rare proposals of real merit favorably reported upon, which from a hasty survey might have been confounded and overlaid with the mass of untried puerilities, no less served to strengthen and assist the Government during its years of greatest trial, need, and exhaustion.

From the outset of the unnatural sectional revolt, fully appreciating the vastness of the interests, the sacrifices, and the dangers involved, Henry contemplated the crisis—not with despondency, but with a profound sorrow and solicitude. While his sympathies and his hopes were all for the preservation of the national integrity of jurisdiction, he was little given to public exhibitions of his feelings. Undemonstrative—less from temperament than from the deliberate and habitual subjection of emotional expression to reason, during those times of feverish excitement apprehension and circumspection necessarily attendant on the prevalence of a gigantic rebellion (unparalleled in incentive, in temper, and in magnitude) many of whose leaders had been among his personal friends, he was not unnaturally looked upon by many as lukewarm in his patriotism, if not disloyal in his citizenship. To the occasional innuendoes of the press, he deigned no answers: he was the last man to accord compliance with the urgency of a popular clamor. And yet during the entire period of the Southern Insurrection, he was the personal and trusted friend of President Lincoln.†

* *Smithsonian Report* for 1864, p. 15.

† Early in the war (in the autumn of 1861,) a caller at the Presidential Mansion very anxious to see the Chief Magistrate of the nation, was informed that he could not then be seen, being engaged in an important private consultation. The caller not to be repulsed, wrote on a piece of paper

CONTRIBUTIONS TO SCIENCE AT WASHINGTON.

In addition to what may be called the public labors of Henry so diligently performed in various fields after his advent to the Smithsonian Institution, it is well briefly to contemplate the special scientific work he was able to accomplish in the intervals of his exacting occupations; that some estimate may be formed of the independent value of his later contributions, as well as of his wonderful industry. While still engaged in his difficult task of organizing and shaping the policy of the Institution, in 1850, on taking occasion to present before the American Association at New Haven, Conn., a résumé of the electrical phenomena exhibited by the Leyden jar, and their true interpretation, he remarked that "for the last three and a half years, all his time and all his thoughts had been given to the details of the business of the Smithsonian Institution. He had been obliged to withdraw himself entirely from scientific research; but he hoped that now the Institution had got under way, and the Regents had allowed him some able assistants, that he would be enabled in part at least to return to his first love—the investigation of the phenomena of nature."*

Thermal Telescope.—Shortly after his establishment at Washington, he continued a series of former experiments with the "thermo-galvanic multiplicator" devised by Nobili and Melloni in 1831; and by some slight but significant modifications of the apparatus, he succeeded in imparting to it a most surprising delicacy of action. With the thermo-electric pile carefully adjusted at the

that he must see Mr. Lincoln personally, on a matter of vital and pressing importance to the public welfare. This of course secured his admission to the presence of Mr. Lincoln, who was sitting with a middle-aged gentleman. Observing the hesitancy of his visitor, the President told him he might speak freely, as only a friend was present. Whereupon the visitor announced that for several evenings past he had observed a light exhibited on the highest of the Smithsonian towers, for a few minutes about nine o'clock, with mysterious movements, which he felt satisfied were designed as signals to the rebels encamped on Munson's Hill in Virginia.

Having gravely listened to this information with raised eyebrows, but a subdued twinkle of the eye, the President turned to his companion, saying "What do you think of that? Professor Henry." Rising with a smile, the person addressed replied, that from the time mentioned, he presumed the mysterious light shone from the lantern of a watchman who was required at nine o'clock each evening to observe and record the indications of the meteorological instruments placed on the tower.

The painful confusion of the officious informant, at once appealed to Henry's sensibility; and quite unmindful of the President, he approached the visitor, offering his hand, and with a courteous regard counselled him never to be abashed at the issue of a conscientious discharge of duty, and never to let the fear of ridicule interfere with its faithful execution.

* *Proceed. Am. Assoc.* 4th Meeting, New Haven, Aug. 1850, p. 378.

focus of a suitable reflector, his "thermal telescope" when directed to the celestial vault, indicated that the heat radiated inward by our atmosphere when clear, is least at the zenith, and increases downward to the horizon; as was to have been inferred from its increasing mass: when directed to clouds, they were found to differ very widely accordingly as they were condensing or being dissipated; some even indicating a less amount of radiation than the surrounding atmosphere. When directed to a horse in a distant field, its animal heat concentrated on the pile, was distinctly made manifest on the galvanometer needle. Even the heat from a man's face at the distance of a mile could be detected; and that from the side of a house at several miles distance.* These and many similar observations demonstrated to sense the inductions of reason, that there is a constant and universal exchange by radiation in straight lines from every object in nature, following the same laws as the palpable emanation from incandescent bodies; and that even when the amplitude of the thermal vibrations (equivalent to the square root of their dynamic energy) is reduced a million fold, its existence may still be distinctly traced.

Henry showed by experiment, that ice could be employed both as a convex lens for converging heat to a focus, and also as a concave mirror for the same purpose: a considerable portion of the incident rays being transmitted, a large portion reflected, and the remainder absorbed by the ice.

He presented to the American Philosophical Society, a discussion of the problem of the suspension of the ball in a water jet or fountain.†

In 1849, for the purpose of estimating the effects of certain meteorological conditions of the atmosphere, he made some experiments on the lateral radiation from a current of ascending heated air at different distances above the flame; the latter being thoroughly eclipsed.

He also experimented on the radiation of heat from a hydrogen flame, which was shown to be quite small, notwithstanding the high temperature of the flame. By placing an infusible and incombustible solid in the flame, while the temperature is much reduced, the radiant light and heat are greatly increased ‡ Results closely analogous to those he obtained in the differences between the audibility of vibrating tuning-forks when suspended by a soft thread, or when rigidly attached to a sounding-board. These results have also an undoubted significance with regard to celestial radiations; not only as to the differences between gaseous nebulæ and stars or clusters, but as to the differences

* *Sill. Am. Jour. Sci.* Jan. 1848, vol. v. pp. 113, 114.
† *Proceed. Am. Phil. Soc.* Oct. 16, 1848, vol. iv. p. 285.
‡ *Proceed. Am. Phil. Soc.* Oct. 19, 1849, vol. v. p. 108.

between stars in a probably different state of condensation or of specific gravity.

A few years later, he continued his investigation of this subject of radiation, more especially with reference to Rumford's "Observations relative to the means of increasing the quantities of Heat obtained in the Combustion of Fuel:" published in Great Britain in 1802.* He found that Rumford's recommendation of the introduction of balls of clay or of fire brick (about two and a half inches in diameter) into a coal fire, was fully justified as an economic measure: more heat being thereby radiated from the fire into the room, and less being carried up the flue. He also showed however that for culinary purposes, while the incandescent or heated clay increases the *radiation*, and thereby improves the quality of the fire for *roasting*, it correspondingly expends the *temperature*, and thereby diminishes its power for *boiling*. "That a solid substance increases the radiation of the heat of a flame, is an interesting fact in connection with the nature of heat itself. It would seem to show that the vibrations of gross matter are necessary to give sufficient intensity of impulse to produce the phenomena of ordinary radiant heat."†

In 1851, he read before the American Association at Albany, a paper "On the Theory of the so-called Imponderables:" (mainly a development of his earlier discussion in 1846, of the molecular constitution of matter,) in which he forcibly criticised a frequent tendency to assume or multiply unknown and unrealizable modes of action: holding that with regard to the most subtle agencies of nature, we have no warrant by the strict scientific method, for resorting to other than the observed and established laws of matter and force, until it has been exhaustively demonstrated that these are insufficient: and that time has not yet come. The fundamental laws of mechanical philosophy "are five in number; viz., the two laws of force—attraction, and repulsion, varying with some function of the distance; and secondly, the three laws of motion—the law of inertia, of the co-existence of motions, and of action and re-action. Of these laws we can give no explanation: they are at present considered as ultimate facts; to which all mechanical phenomena are referred, or from which they are deduced by logical inference. The existence of these laws as has been said, is deduced from the phenomena of the operations of matter in masses; but we apply them by analogy to the minute and invisible portions of matter which constitute the atoms or molecules of gases, and we find that the inferences from this assumption are borne out by the results of experience." He regarded the modern kinetic or dynamic theory of gases, by its

* *Journal Royal Institution*, 1802, vol. i. p. 28.

† *Proceed. Am. Assoc.* Providence, Aug. 1855, pp. 112–116. "On the Effect of mingling Radiating substances with Combustible materials."

predictions and verifications, as furnishing almost a complete establishment of the atomic and molecular theory of matter. Referring to the ingenious hypothesis of Boscovich, he thought that though well adapted to embrace the two static laws above mentioned, it did not appear equally well adapted to satisfy in any intelligible sense the three kinetic laws. He contended that every attempt at conforming our conception of the ultimate constitution of matter to the inductions of experience, would seem to conduct us directly to the atomic hypothesis of Newton. A careful study of the dynamics of the so-called "imponderables" certainly tended to their unification. Admitting the difficulty of framing an entirely satisfactory theory of the resultant transverse action of electricity, he suggested that a tangential force was not accordant with any inductions from direct experience; and was incapable of direct mechanical realization. Extending the atomic conception of matter to the ætherial medium of space, he concluded by urging "the importance in the adoption of mechanical hypotheses, of conditioning them in strict accordance with the operations of matter under the known laws of force and motion, as exhibited in time and space."*

Among the various public Addresses delivered by Henry on special occasions, reference may be here made to his excellent exposition of the nature of power, and the functions of machinery as its vehicle, concluding with a sketch of the progress of art, pronounced at the close of the Exhibition of the Metropolitan Mechanics' Institute, in Washington, on the evening of March 19th, 1853. After representing to his hearers the close physical analogy between the human body as a moving machine, and the steam locomotive under an intelligent engineer, he remarked: "In both, the direction of power is under the influence of an immaterial, thinking, willing principle, called the soul. But this must not be confounded as it frequently is with the motive power. The soul of a man no more moves his body, than the soul of the engineer moves the locomotive and its attendant train of cars. In both cases the soul is the directing, controlling principle; not the impelling power."†

Views of Education.—Another address deserving of special notice (delivered the following year,) is his introductory discourse before the "Association for the Advancement of Education," as its retiring President. In this, he maintained that inasmuch as "the several faculties of the human mind are not simultaneously developed, in educating an individual we ought to follow the order of nature, and to adapt the instruction to the age and mental stature of the pupil. Memory, imitation,

* *Proceed. Am. Assoc.* Albany, Aug. 1851, pp. 84–91.
† *Closing Address* Metr. Mech. Inst. Washington, 1853, p. 19

imagination, and the faculty of forming mental habits, exist in early life, while the judgment and the reasoning powers are of slower growth." Hence less attention should be given to the development of the reasoning faculties, than to those of observation: the juvenile memory should be stored rather with facts, than with principles: and he condemned as mischievous "the proposition frequently advanced, that the child should be taught nothing but what it can fully comprehend, and the endeavor in accordance with this, to invert the order of nature, and attempt to impart those things which cannot be taught at an early age, and to neglect those which at this period of life, the mind is well adapted to receive. By this mode we may indeed produce remarkably intelligent children, who will become remarkably feeble men. The order of nature is that of art before science; the entire concrete first, and the entire abstract last. These two extremes should run gradually into each other, the course of instruction becoming more and more logical as the pupil advances in years."—' The cultivation of the imagination should also be considered an essential part of a liberal education: and this may be spread over the whole course of instruction, for like the reasoning faculties the imagination may continue to be improved until late in life."

Applying this same reasoning to the moral training of youth, he considered that (as in the intellectual culture) the object should be "not only to teach the pupil how to *think*, but how to *act* and to *do;* placing great stress upon the early education of the habits. . . . We are frequently required to act from the impulse of the moment, and have no time to deduce our course from the moral principles of the act. An individual can be educated to a strict regard for truth, to deeds of courage in rescuing others from danger, to acts of benevolence, generosity, and justice. . . . The future character of a child and that of the man also, is in most cases formed probably before the age of seven years. Previously to this time impressions have been made which shall survive amid the vicissitudes of life, amid all the influences to which the individual may be subjected, and which will outcrop as it were, in the last stage of his earthly existence, when the additions to his character made in later years, have been entirely swept away." Childhood (he intimated) is less the parent of manhood, than of age: the special vices of the individual child though long subdued, sometimes surviving and re-appearing in his "second childhood."

Affirming that culture is constraint,—education and direction an expenditure of force, and extending his generalization from the individual to the race, he controverted the idea so popular with some benevolent enthusiasts, that there is a spontaneous tendency in man to civilization and advancement. The origins of past civilizations—taking a comprehensive glance at far dis-

tant human populations—have been sporadic as it were, and their prevalence comparatively transitory. "It appears therefore that civilization itself may be considered as a condition of unstable equilibrium, which requires constant effort to be sustained, and a still greater effort to be advanced. It is not in my view the 'manifest destiny' of humanity to improve by the operation of an inevitable necessary law of progress: but while I believe that it is the design of Providence that man should be improved, this improvement must be the result of individual effort, or of the combined effort of many individuals animated by the same feeling and co-operating for the attainment of the same end. . . . If we sow judiciously in the present, the world will assuredly reap a beneficent harvest in the future: and he has not lived in vain, who leaves behind him as his successor, a child better educated—morally, intellectually, and physically, than himself. From this point of view, the responsibilities of life are immense. Every individual by his example and precept, whether intentionally or otherwise, does aid or oppose this important work, and leaves an impress of character upon the succeeding age, which is to mould its destiny for weal or woe, in all coming time. . . . The world however is not to be advanced by the mere application of truths already known: but we look forward (particularly in physical science) to the effect of the development of new principles. We have scarcely as yet read more than the title-page and preface of the great volume of nature, and what we do know is as nothing in comparison with that which may be yet unfolded and applied."*

Experiments on Building-stone.—In 1854, a series of experiments on the strength of different kinds of building-stone, was undertaken by Henry as one of a commission appointed by the President, having reference to the marbles offered for the extension of the United States Capitol. Specimens of the different samples—accurately cut to cubical blocks one inch and a half in height, were first tried by interposing a thin sheet of lead at top and bottom, between the block and the steel plates of the

* *Proceed. Assoc. Adv. Education*, 4th Session, Washington, Dec. 28, 1854, pp. 17-31. The pregnant thought that human civilization is an artificial and coerced condition, would seem to have a suggestive bearing on the two great theories of *development* and *evolution*, so generally confounded by the superficial. What may be called the radical difference between these two views of organic extension, is that the former assumes an inherent mysterious tendency to progression, whose motto is ever "excelsior;" while the latter assumes a general tendency to variation within moderate limits in indefinite directions; so that elevation is no more normal than degradation, and indeed may be regarded as rarer and more exceptional, since at every upward stage attained by the few, there are probably more further digressions downward than upward, the motto being ever "aptior."

crushing dynamometer. "This was in accordance with a plan adopted by Rennie, and that which appears to have been used by most if not all of the subsequent experimenters in researches of this kind. Some doubt however was expressed as to the action of interposed lead, which induced a series of experiments to settle this question; when the remarkable fact was discovered that the yielding and approximately equable pressure of the lead caused the stone to give way at about half the pressure it would sustain without such an interposition. For example, one of the cubes precisely similar to another which withstood a pressure of upwards of 60,000 pounds when placed in immediate contact with the steel plates, gave way at about 30,000 pounds with lead interposed. This interesting fact was verified in a series of experiments embracing samples of nearly all the marbles under trial, and in no case did a single exception occur to vary the result.

"The explanation of this remarkable phenomenon (now that the fact is known) is not difficult. The stone tends to give way by bulging out in the centre of each of its four perpendicular faces, and to form two pyramidal figures with their apices opposed to each other at the center of the cube, and their bases against the steel plates. In the case where rigid equable pressure is employed, as in that of the thick steel plate, all parts must give way together. But in that of a *yielding* equable pressure as in the case of interposed lead, the stone first gives way along the outer lines or those of least resistance, and the remaining pressure must be sustained by the central portions around the vertical axis of the cube. After this important fact was clearly determined, lead and all other interposed substances were discarded, and a method devised by which the upper and lower surfaces of the cube could be ground into perfect parallelism. . . . All the specimens tested were subjected to this process, and on their exposure to pressure were found to give concordant results. The crushing force sustained was therefore much greater than that heretofore given for the same material."*

In the same communication, interesting remarks are made on the *tensile* strength of materials, particularly the metals. "According to the views presented, the difference in the tenacity in steel and lead does not consist in the attractive cohesion of the atoms, but in their capability of slipping upon each other:" that is on the difference of lateral *adhesion* of the molecules, as exemplified in ice and water. A bar of soft metal—as lead—subjected to tensile strain, by reason of the greater freedom of the exterior layers of molecules, exhibits a stretching and thinning; while the interior molecules being more confined by the surrounding pressure, are less mobile, permit less elongation

* *Proceed. Am. Assoc.* Providence, Aug. 1855, pp. 102–112.

of the mass, and are therefore the first to commence breaking apart. Accordingly on ultimate separation, each fragment exhibits a hollow or cup-like surface of fracture, where the interior portion of the material has first parted: the depth of the concavity being somewhat proportioned to the malleability of the substance. "With substances of greater rigidity, this effect is less apparent, but it exists even in iron, and the interior fibres of a rod of this metal may be entirely separated, while the outer surface presents no appearance of change. From this it would appear that metals should never be elongated by mere stretching, but in all cases by a process of wire-drawing, or rolling. A wire or bar must always be weakened by a force which permanently increases its length without at the same time compressing it."*

Hydrometric Experiment.—A novel project for the rectification of spirits by the simple process of static separation of the alcohol and water by the stress of their specific gravities when exposed in long columns, produced in 1854 a considerable sensation. It was alleged that the coercitive compression exerted by the water in a long hydrostatic column greatly accelerated the displacement and separation induced by gravitation, and that only a few hours were necessary to complete the process, if the column were sufficiently high.†

A patent was obtained: affidavits and samples fully attested the wonderful efficiency of the process; and only the co-operation of confiding capitalists was required, to realize fabulous profits, and effect a manufacturing and commercial revolution.

Simply in the interests of truth, Henry undertook the careful investigation of this surprising pretension. One of the towers of the Smithsonian Building supplied a convenient well for the experiment, easily accessible throughout its height. "A series of stout iron tubes of about an inch and a half internal diameter formed the column; the total length of which was one hundred and six feet. Four stop-cocks were provided; one at the bottom, one about four feet from the top, and the other two to the intermediate space equally divided or nearly so." Very careful hydrometer and thermometer registers were made at increasing intervals of time, the last being that of nearly half a year: a portion of the reserved liquor being simultaneously tested. The result stated, is: "There is not the slightest indication of any

* This conclusion is not at all in opposition to the ascertained fact of the increased strength imparted to an iron rod by thermo-tension, discovered by Prof. Walter R Johnson.

† An incidental remark in Gmelin's "Handbook of Chemistry" seemed to give some color of plausibility to the scheme. "Brandy kept in casks is said to contain a greater proportion of spirit in the upper, and of water in the lower part." Gmelin's *Handbook*, Translated by Henry Watts. London, 1841, part i. sect. 4,—vol. i. p. 112.

difference of density between the original liquor and that from the top or bottom of the column, after the lapse of hours, days, weeks, or months. The fluid at the bottom of the tube it must be remembered was for five months exposed to the pressure of a column of fluid at least one hundred feet high." *

Sulphuric-acid Barometer.—In 1856, Henry had constructed for the Smithsonian Institution, at the suggestion of Professor G. C. Schaeffer, a large sulphuric acid barometer, whose column being more than seven times the height of the mercurial column (about 18½ feet) gave correspondingly enlarged and sensitive indications. Water barometers with cisterns protected by oil, (as that constructed by Daniell for the Royal Society,) have always proved instable. With reference to sulphuric acid, "The advantages of this liquid are: 1st that it gives off no appreciable vapor at any atmospheric temperature; and 2nd that it does not absorb or transmit air. The objections to its use are: 1st the liability to accident from the corrosive nature of the liquid, either in the filling of the tube or in its subsequent breakage; and 2nd its affinity for moisture, which tends to produce a change in specific gravity." The latter defect was obviated by a drying apparatus consisting of a tubulated bottle containing chloride of calcium, and connected by a tube with the glass bottle forming the reservoir, which excluded all moisture from the transmitted air. "The glass tube [of the barometer] is two hundred and forty inches long, and three-fourths of an inch in diameter; and is enclosed in a cylindrical brass case of the same length, and two and a half inches in diameter. The glass tube is secured in the axis of the brass case by a number of cork collars, placed at intervals."† This barometer continued in successful and satisfactory use for many years; and had its readings constantly recorded.

Of several of Henry's courses of experiments, no details have been published; and his original notes appear to have perished. In 1861, he made a number of experiments on the effects of burning gunpowder in a vacuum, as well as in different gases.

"A series of researches was also commenced, to determine more accurately than has yet been done, the expansion produced in a bar of iron at the moment of magnetization of the metal by means of a galvanic current. The opportunity was taken with the consent of Professor Bache, of making these experiments with the delicate instruments which had previously been employed in determining the varying length, under different temperatures, of the measuring apparatus of the base lines of the United States Coast Survey."‡ This wonderfully microscopic measuring appa-

* *Proceed. Am. Assoc.* Providence, Aug. 1855, pp. 142, 143.
† *Proceed. Am. Assoc.* Albany, Aug. 1856, pp. 135–138.
‡ *Smithsonian Report* for 1861, p. 38.

ratus devised by Mr. Joseph Saxton, was capable of distinguishing by the light-ray index of its contact reflector, a dimension equal to a half wave-length of average light, or the 100,000th part of an inch. The long under-ground vaults of the Smithsonian building having been selected as a suitable place for the precise verification of the residual co-efficient of compensated temperature expansion of the base rods of the Survey, the opportunity was seized by Henry, at the termination of the investigation, to apply the same delicate apparatus to the determination of the polarized or magnetic expansion.

In less than six years from the time of these researches, he was called on to mourn the death of his life-long intimate and honored friend, who had always exhibited so fraternal a sympathy and co-operation with his own varied labors. In consequence of this event—the death of his friend Professor A. Dallas Bache in 1867, Henry was chosen in 1868, to be his successor as President of the National Academy of Sciences. At the request of that body, he prepared a eulogy of his friend the late President, which was read before the Academy April 16th, 1869. In grateful acknowledgement of the wise counsels and valuable services of Dr. Bache as one of the Regents of the Smithsonian Institution for nearly twenty years, he observed: "To say that he assisted in shaping the policy of the establishment would not be enough. It was almost exclusively through his predominating influence that the policy which has given the Institution its present celebrity, was after much opposition finally adopted. . . . Nor would it be possible for him [the speaker] to abstain from acknowledging with heart-felt emotion, that he was from first to last supported and sustained in his difficult position by the fraternal sympathy, the prudent counsel, and the unwavering friendship of the lamented deceased."*

Many minor contributions in various fields of scientific observation, must here be omitted: but it would be inexcusable, in this place and on this occasion, to neglect a reference to the active part he took in the organization and advancement of this Society;† and the unflagging interest ever exhibited in its proceedings, from the date of its convocation, March 13th, 1871, to that of his last illness. All here remember with what punctuality he attended the meetings—whether of the executive committee or of the society, undeterred by inclemencies of the weather which kept away many much younger members. All

* *Biographical Memoirs, Nat. Acad. Sci.* vol. i. pp. 181–212. Republished in the *Smithsonian Report* for 1870, pp. 91–116. The father of Prof. Bache—Richard Bache, was a son of the only daughter of the illustrious Benjamin Franklin.

† The Philosophical Society of Washington.

here, recall with what unpretentious readiness he communicated from his rich stores of well-digested facts, observations—whether initiatory or supplementary, on almost every topic presented to our notice; how apt his illustrations and suggestions in our spontaneous discussions; and with what unfailing interest we ever listened to his words of exposition, of knowledge, and of wisdom: utterances which we shall never hear again; and which unwritten and unrecorded, have not been even reported in an abstract.

Range of information.—It was not alone in those physical branches of knowledge to which he had made direct original contributions, that the mental activities of Henry were familiarly exercised and conspicuously exhibited. There was scarcely a department of intellectual pursuit in which he did not feel and manifest a sympathetic interest, and in which he did not follow with appreciative grasp its leading generalizations. Holding ever to the unity of Nature as the expression and most direct illustration of the Unity of its Author, he believed that every new fact discovered in any of nature's fields, would ultimately be found to be in intimate correlation with the laws prevailing in other fields—seemingly the most distant.* To his large comprehension, nothing was insignificant, or unworthy of consideration. He ever sought however to look beyond the ascertained and isolated or classified fact, to its antecedent cause; and in opposition to the dogma of Comte, he averred that the knowledge of facts is not *science*,—that these are merely the materials from which its temple is constructed by sagacious and attested speculation.

Among his earlier studies, Chemistry occupied a prominent place. The youthful assistant in the laboratory of his former instructor and ever honored friend, Dr. T. Romeyn Beck, and later, himself a teacher of the art and knowledge to others, a skilful manipulator, an acute analyst and investigator of re-actions, he seemed at first destined to become a leader in chemical research. Like Newton, he endeavored to bring the atomic combinations under the conception of physical laws; believing this essential to the development of chemistry as a true science. He always kept himself well informed on the progress of the more recent doctrines of quantivalence, and the newer system of nomenclature.

He had also paid considerable attention to geology; with its relations to palæontology on the one side, and to physical geography on the other.

Familiar with the details—as well of astronomical observation

* "A proper view of the relation of science and art will enable him [the reader] to see that the one is dependent on the other; and that each branch of the study of nature is intimately connected with every other." (*Agricultural Report* for 1857, p. 419.)

as of the mathematical processes of reduction, he would have done honor to any Observatory placed under his charge. He was lenient in his judgment of the ancient star-worshippers; and was always greatly attracted by astronomical discoveries.

Well read in the science of Political Economy, he had by observation and analysis of human nature, made its inductive principles his own, and had satisfied himself that its deductions were fully confirmed by an intelligent appreciation of the teachings of financial history. He attributed the lamentable disregard of its fundamental doctrines, by many so-called legislators, to a want of scientific training, and consequent want of perception and of faith in the dominion and autonomy of natural law.

A good linguist, he watched with appreciative interest the progress of comparative philology, and the ethnologic significance of its generalizations, in tracing out the affiliations of European nations. By no means neglectful of lighter literature, he enjoyed at leisure evenings, in the bosom of his cultivated family, the readings of modern writers, and the suggestive interchange of sentiment and criticism. Striking passages of poetry made a strong impression on his retentive memory; and it was not unusual to hear him embellish some graver fact, in conversation, with an unexpected but most apt quotation. With a fine æsthetic feeling, his appreciation and judgment of works of art, were delicate and discriminating.

He held very broad and decided views as to the reign of order in the Cosmos. Defining science as the "knowledge of natural law," and law as the "will of God," Henry was always accustomed to regard that orderly sequence called the "law," as being fixed and immutable as the providence of its Divine Author: admitting in no case caprice or variableness. The doctrine of the absolute dominion of law—so oppressive and alarming to many excellent minds, was to him accordingly but a necessary deduction from his theologic and religious faith.

The series of meteorological essays already referred to as contributed to the Agricultural Reports of the Commissioner of Patents, commences with this striking passage, "All the changes on the surface of the earth and all the movements of the heavenly bodies, are the immediate results of natural forces acting in accordance with established and invariable laws; and it is only by that precise knowledge of these laws, which is properly denominated science, that man is enabled to defend himself against the adverse operations of Nature, or to direct her innate powers in accordance with his will. At first sight, it might appear that meteorology was an exception to this general proposition, and that the changes of the weather and the peculiarities of climate in different portions of the earth's surface, were of all things the most uncertain and farthest removed from the dominion

of law: but scientific investigation establishes the fact that no phenomenon is the result of accident, or even of fitful volition. The modern science of statistics has revealed a permanency and an order in the occurrence of events depending on conditions in which nothing of this kind could have been supposed. Even those occurrences which seem to be left to the free will, the passion, or the greater or less intelligence of men, are under the control of laws—fixed, immutable, and eternal." And after dwelling on the developments and significance of moral statistics, he adds: "The astonishing facts of this class lead us inevitably to the conclusion that all events are governed by a Supreme Intelligence who knows no change; and that under the same conditions, the same results are invariably produced."*

Organic Dynamics.—The contemplation of these uniformities leads naturally to the great modern generalization of the correlation of all the working energies of nature: and this to the subject of organic dynamics. "Modern science has established by a wide and careful induction, the fact that plants and animals consist principally of solidified air; the only portions of an earthy character which enter into their composition, being the ashes that remain after combustion." Some ten years before this, or in 1844, (as already noticed in an earlier part of this memoir) Henry had very clearly indicated the correlation between the forces exhibited by inorganic and organic bodies: arguing that from the chemical researches of Liebig, Dumas, and Boussingault, "it would appear to follow that animal power is referable to the same sources as that from the combustion of fuel:"† probably the earliest explicit announcement of the now accepted view. In the series of agricultural essays above referred to, he endeavored to frame more definitely a chemico-physical theory by which the elevation of matter to an organic combination in a higher state of power than its source, might be accounted for. Regarding "vitality" not as a mechanical force, but as an inscrutable *directing* principle resident in the minute germ—supposed to be vegetative, and enclosed in a sac of starch or other organic nutriment, he considered the case of such provisioned germ (a bean or a potato) embedded in the soil, supplied with a suitable amount of warmth and moisture

* *Agricultural Report Com. Pat.* for 1855, p. 357.

† *Proceed. Am. Phil. Soc.* Dec. 1844, vol. iv. p. 129. The admirable treatise of Dr. Julius R. Mayer of Heilbronn, on "Organic Movement in its relation to material changes," in which for the first time he maintained the thesis that all the energies developed by animal or vegetable organisms, result from internal changes having their dynamic source in external forces, was published the following year, or in 1845. Rumford nearly half a century earlier, had a partial grasp of the same truth. (*Phil. Trans. R. S.* Jan. 25, 1798, vol. lxxxviii. pp. 80–102.)

to give the necessary molecular mobility, soon sending a rootlet downward into the earth, and raising a stem toward the surface, furnished with incipient leaves. Supposing the planted seed to be a potato, on examination we should find its large supply of starch exhausted, and beyond the young plant, nothing remaining but the skin, containing probably a little water. What has become of the starch? "If we examine the soil which surrounded the potato, we do not find that the starch has been absorbed by it; and the answer which will therefore naturally be suggested, is that it has been transformed into the material of the new plant, and it was for this purpose originally stored away. But this though in part correct, is not the whole truth: for if we weigh a potato prior to germination, and weigh the young plant afterward, we shall find that the amount of organic matter contained in the latter, is but a fraction of that which was originally contained in the former. We can account in this way for the disappearance of a *part* of the contents of the sac, which has evidently formed the pabulum of the young plant. But here we may stop to ask another question: By what power was the young plant built up of the molecules of starch? The answer would probably be, by the exertion of the vital force: but we have endeavored to show that vitality is a *directing principle*, and not a mechanical power, the expenditure of which does work. The conclusion to which we would arrive will probably now be anticipated. The portion of the organic molecules of the starch, etc., of the tuber, as yet unaccounted for, has run down into inorganic matter, or has entered again into combination with the oxygen of the air, and in this running down and union with oxygen, has evolved the power necessary to the organization of the new plant. . . . We see from this view that the starch and nitrogenous materials in which the germs of plants are imbedded, have two functions to fulfil, the one to supply the pabulum of the new plant, and the other to furnish the power by which the transformation is effected, the latter being as essential as the former. In the erection of a house, the application of mechanical power is required as much as a supply of ponderable materials."*

† *Agricultural Report*, for 1857, pp. 440–444. In May, 1842, Dr. Julius R. Mayer published in Liebig's *Annalen der Chemie*, etc., his first remarkable paper on "The Forces of Inorganic Nature," constituting the earliest scientific ennunciation of the correlation of the physical forces; and (if we except the work of Seguin in 1839,) of the mechanical equivalent of heat. (*Annalen u.s.w.* vol. xlii. pp. 233–240.) In September, 1849, Dr. R. Fowler read a short paper before the British Association at Birmingham, on "Vitality as a Force correlated with the Physical Forces." (*Report Brit. Assoc.* 1849, part ii. pp. 77, 78.) In June, 1850, Dr. W. B. Carpenter presented to the Royal Society a much fuller memoir "On the Mutual Relations of the Vital and Physical Forces." (*Phil. Trans. R. S.* vol. cxl. pp. 727–757.) Neither of these essays accounts for the

The less difficult problem of the building up of the plant after the consumption of the seed, under the direct action of the solar rays, is then considered; the leaves of the young plant absorbing by their moisture carbonic acid from the atmosphere, which being decomposed by solar actinism, yields the de-oxidized carbon to enter into the structure of the organism. "All the material of which a tree is built up, (with the exception of that comparatively small portion which remains after it has been burnt, and constitutes the ash,) is derived from the atmosphere. In the decomposition of the carbonic acid by the chemical ray, a definite amount of power is expended, and this rémains as it were locked up in the plant so long as it continues to grow." And thus under the expenditure of an external force, the plant (whether the annual cellular herb or the perennial fibrous tree) was shown to be built up from the simpler stable binary compounds of the inorganic world, to the more complex and unstable ternary compounds of the vegetable world. "In the *germination* of the plant, a part of the organized molecules runs down into carbonic acid to furnish power for the new arrangement of the other portion. In this process no extraneous force is required: the seed contains within itself the power, and the material, for the growth of the new plant up to a certain stage of its development. Germination can therefore be carried on in the dark, and indeed the chemical ray which accompanies light retards rather than accelerates the process." (p. 446.) This important organic principle appears to receive here its earliest enunciation.

It was also pointed out that on the completion of the cycle of growth (however brief or however extended), the decay of the plant not only returns the elevated matter to its original lower plane, but equally returns the entire amount of heat energy absorbed in its elevation: an amount precisely the same, whether the slow oxidation be continued through a series of years, or a rapid combustion be completed in as many minutes. "The power which is given out in the whole descent is according to the dynamic theory, just equivalent to the power expended by the impulse from the sun in elevating the atoms to the unstable condition of the organic molecules. If this power is given out in the form of vibrations of the ætherial medium constituting heat, it will not be appreciable in the ordinary decay say of a tree, extending as it may through several years: but if the process be rapid, as in the case of combustion of wood, then the same amount of power will be given out in the energetic form of heat of high intensity."

The elevation of inorganic matter (carbonic acid, water, and

amount of building energy displayed in the development of the seed, under conditions of low and diffused heat: and the expression "Vital Force" used both by Fowler and Carpenter, was studiously avoided by Henry.

ammonia,) to the vegetable plane of power, introduces naturally the consideration of the still higher elevation of vegetable organic matter to the animal plane of power. "As in the case of the seed of the plant, we presume that the germ of the future animal pre-exists in the egg; and that by subjecting the mass to a degree of temperature sufficient perhaps to give greater mobility to the molecules, a process similar in its general effect to that of the germination of the seed commences. . . . During this process, power is evolved within the shell, we cannot say in the present state of science under what particular form; but we are irresistibly constrained to believe that it is expended under the direction again of the vital principle, in re-arranging the organic molecules, in building up the complex machinery of the future animal, or developing a still higher organization, connected with which are the mysterious manifestations of thought and volition. In this case as in that of the potato, the young animal as it escapes from the shell, weighs less than the material of the egg previous to the process of incubation. The lost material in this case as in the other, has run down into an inorganic condition by combining with oxygen, and in its descent has developed the power to effect the transformation we have just described." The consumption of internal power does not however stop with the development of the young animal, as it does in the case of the young plant. "The young animal is in an entirely different condition: exposure to the light of the sun is not necessary to its growth or its existence: the chemical ray by impinging on the surface of its body does not decompose the carbonic acid which may surround it, the conditions necessary for this decomposition, not being present. It has no means by itself to elaborate organic molecules; and is indebted for these entirely to its food. It is necessary therefore that it should be supplied with food consisting of organized materials; that is of complex molecules in a state of power. . . . The power of the living animal is immediately derived from the running down of the complex organized molecules of which the body is formed, into their ultimate combination with oxygen, in the form of carbonic acid and water, and into ammonia. Hence oxygen is constantly drawn into the lungs, and carbon is constantly evolved. . . . The animal is a curiously contrived arrangement for burning carbon and hydrogen, and for the evolution and application of power. A machine is an instrument for the application of power, and not for its creation. The animal body is a structure of this character. . . . A comparison has been made between the work which can be done by burning a given amount of carbon in the machine—man, and an equal amount in the machine—steam-engine. The result derived from an analysis of the food in one case, and the weight of the fuel in the other, and these compared with the quantity of water raised by each to a known elevation,

gives the relative working value of the two machines. From this comparison, made from experiments on soldiers in Germany and France, it is found that the human machine in consuming the same amount of carbon, does four and a half times the amount of work of the best Cornish engine."

"There is however one striking difference between the animal body and the locomotive machine, which deserves our special attention; namely the power in the body is constantly evolved by burning (as it were,) parts of the materials of the machine itself; as if the frame and other portions of the wood-work of the locomotive were burnt to produce the power, and then immediately renewed. The voluntary motion of our organs of speech, of our hands, of our feet, and of every muscle in the body, is produced not at the expense of the soul but at that of the material of the body itself. Every motion manifesting life in the individual, is the result of power derived from the death as it were of a part of his body. We are thus constantly renewed and constantly consumed; and in this consumption and renewal consists animal life."*

Seven years after the publication of this highly original and suggestive exposition, (whose topics and line of discussion had been distinctly formulated and sketched out more than two years before, at the commencement of the series in 1855,) the eminent physiologist Dr. Carpenter produced his valuable memoir on the Conservation of Force in Physiology; in which he for the first time distinctly affirms the development of vegetative reproductive energy, by the partial running down of matter to its stabler compounds,—"by the retrograde metamorphosis of a portion of the organic compounds prepared by the previous nutritive operations:" and also the ultimate return by decay, of the whole amount of force as well as of matter, temporarily borrowed from nature's store. Likewise with animal powers, "these forces are developed by the retrograde metamorphosis of the organic compounds generated by the instrumentality of the plant, whereby they ultimately return to the simple binary forms (water, carbonic acid, and ammonia,) which serve as the essential food of vegetables. . . Whilst the vegetable is constantly engaged (so to speak) in raising its component materials from a lower plane to the higher, by means of the power which it draws from the solar rays,—the animal whilst raising one portion of these to

* *Agricultural Report* for 1857, pp. 445–449. This important essay it will be observed, antedates Prof. Joseph Le Conte's paper "On the Correlation of Physical, Chemical, and Vital Force," read before the American Association at Springfield, Aug. 1859, (*Proceed. Am. Assoc.* pp. 187–203: and Sill. *Am. Jour. Sci.* Nov. 1859, vol. xxviii. pp. 305–319,) as well as Dr. Carpenter's second and more mature paper "On the application of the Principle of Conservation of Force to Physiology," published in Crookes' *Quarterly Journal of Science*, for Jan. and April, 1864, (vol. i. pp. 76–87; and pp. 259–277.)

a still higher level by the descent of another portion to a lower, ultimately lets down the whole of what the plant had raised."* So little was Henry's earlier paper known abroad, that his name does not occur in Dr. Carpenter's dissertation.

With regard to the great biologic question of the past fifteen years—the affiliation of specific forms, it was impossible that Henry should remain an unconcerned observer. Brought up (as it may be said) in the school of Cuvier, but slightly impressed with the brilliant previsions of his competitor, Geoffroy Saint Hilaire, accustomed to look upon the recurrent hypotheses of automatic development as barren speculations, and beside all this, ever the warmly attached personal friend of Agassiz, he approached the consideration of this controverted subject, certainly with no antecedent affirmative pre-possessions. His general acquaintance with the ascertained facts of the metamorphic development of the individual organism from its origin, as well as with the remarkable analogies and homologies disclosed by the sciences of comparative physiology and embryology, served however in some measure to prepare his mind to apprehend the significance of the indications which had been so industriously collected, and so intelligently collated: and from the very first, he accepted the problem as a purely philosophical one; employing that much abused term in no restricted sense. With no more reserve in the expression of his views, than the avoidance of unprofitable controversies, (though no one more than he—enjoyed the calm and purely intellectual discussion of an unsettled question by its real *experts*,) he yet found no occasion to write upon the subject. The unpublished opinions however, of one so wise and eminent, cannot be a matter of indifference to the student of nature; and their exposition cannot but assist to enlighten our estimate of the mental stature of the man, and of his breadth of apprehension and toleration.

Whatever may be the ultimate fate of the theory of natural selection, (he remarked in the freedom of oral intercourse with several naturalists,) it at least marks an epoch, the first elevation of natural history (so-called) to the really scientific stage: it is based on induction, and correlates a large range of apparently disconnected observations, gathered from the regions of palæontology or geological successions of organisms, their geographical distribution, climatic adaptations and remarkable re-adjustments, their comparative anatomy, and even the occurrence of abnormal variations, and of rudimentary structures—seemingly so uselessly displayed as mere simulations of a "type." It forms a good "working hypothesis" for directing

* *Quart. Jour. Sci.* 1864, vol. i. pp. 86 and 267.

the investigations of the botanist and zoologist.* Natural selection indeed—no less than artificial (he was accustomed to say), is to a limited extent a fact of observation; and the practical question is to determine approximately its reach of application, and its sufficiency as an actual agency, to embrace larger series of organic changes lying beyond the scope of direct human experience. It is for the rising generation of conscientious zoologists and botanists to attack this problem, and to ascertain if practicable its limitations or modifications.

These broad and fearless views, entertained and expressed as early as 1860, or 1861, exhibiting neither the zealous confidence of the votary, nor the jealous anxiety of the antagonist, received scarcely any modification during his subsequent years. Nor did it ever seem to occur to him that any reconstruction of his religious faith was involved in the solution of the problem. So much religious faith indeed was exercised by him in every scientific judgment, that he regarded the teachings of science but as revelations of the Divine mode of government in the natural world: to be diligently sought for and submissively accepted; with the constant recognition however of our human limitations, and the relativity of human knowledge.† Not inappropriately may be here recalled a characteristic statement of the office of hypothesis, made by him some ten years earlier: presenting a consideration well calculated to restrain dogmatism—whether in science or in theology. "It is not necessary that an hypothesis be absolutely true, in order that it may be adopted as an expression of a generalization for the purpose of explaining and predicting new phenomena: it is only necessary that it should be well conditioned in accordance with known mechanical principles. . . . Man with his finite faculties cannot hope in this life to arrive at a knowledge of absolute truth: and were the true theory of the universe, or in other words the precise mode in which Divine Wisdom operates in producing the phenomena of the material world revealed to him, his mind would be unfitted for its reception. It would be too simple in its expression, and too general in its application, to be understood and applied by intellects like ours."‡

* "In the investigation of nature, we provisionally adopt hypotheses as antecedent probabilities, which we seek to prove or disprove by subsequent observation and experiment: and it is in this way that science is most rapidly and securely advanced." (*Agricult. Report*, 1856, p. 456.)

† With reference to the intimations of the comparative antiquity of man, Henry quoted with sympathetic approbation the sentiment so well expressed by the Bishop of London in a Lecture at Edinburgh, that "The man of science should go on honestly, patiently, diffidently, observing and storing up his observations, and carrying his reasonings unflinchingly to their legitimate conclusions, convinced that it would be treason to the majesty at once of science and of religion, if he sought to help either by swerving ever so little from the straight line of truth." (*Smithsonian Report* for 1868, p. 33.)

‡ *Proceed. Am. Assoc.* Albany, Aug. 1851, pp. 86, 87.

INVESTIGATIONS IN ACOUSTICS.

During the last quarter of a century, among the many interests which demanded and engaged his attention, Henry studied with much care various phenomena of acoustics, and added much to our practical as well as theoretical knowledge of this important instrumentality. In 1851, he read a communication before the American Association, "On the Limit of Perceptibility of a direct and reflected Sound," in which he gave as the result of experimental observations, the subjective fact that a wall or other reflecting surface if beyond the distance of about 35 feet from the ear, or from the origin of the sound, gives a distinguishable echo from the sound; but that if the ear or the sounding agent be placed within this distance, the reflected sound appears to blend completely with the original one. From a number of experiments, he found that under the same circumstances, this limit of perceptibility did not vary more than a single foot; but that under differing conditions the limit of distance ranged from 30 to 40 feet, (equivalent to a difference of from 60 to 80 feet of sound travel,) depending partly on the sharpness or clearness of the sound, and partly on the pitch or the length of the soniferous wave, which affected the amount of overlapping of the two series. These results imply a duration of acoustic impression on the ear of about one-sixteenth of a second; serving to show that 16 vibrations to the second must be about the lower limit of a recognizable musical tone.* As applied to lecture-rooms, he pointed out that the ceiling should not be more than about thirty feet high, within which elevation, a smooth ceiling would tend to re-inforce the sound of a speaker's voice.†

Many experiments were afterward made on the resonance of different materials, by means of tuning forks. While a tuning fork suspended by a fine thread continued to vibrate for upward of four minutes with scarcely any appreciable sound, if placed in contact with the top of a pine table, the same vibration continued but ten seconds, but gave a loud full tone. On a marble topped table the sound was much more feeble, and the vibration continued nearly two minutes. While the tuning fork against a brick wall gave a feeble tone continuing for 88 seconds, against a lath and plaster partition it gave a sound considerably louder but continu-

* This does not seem to agree with results obtained by Savart some twenty years previously; who concluded from observations with the siren, "that sounds are distinctly perceptible, and even strong when composed of no more than eight vibrations in a second." (*Rev. Encycl.* July, 1832. Quoted in Sill. *Am. Jour. Sci.* for 1832, vol. xxii. p. 374.) This latter determination is somewhat difficult to reconcile with ordinary observations, as it is certain that intervals of one-eighth of a second would give a very appreciable rattle to almost every ear.

† *Proceed. Am. Assoc.* Cincinnati, May, 1851, pp. 42, 43.

ing only 18 seconds. On a large block of soft India-rubber resting on the marble slab, the vibration was very rapidly extinguished, but without giving any sensible sound. This anomaly required an explanation. By means of a compound wire of copper and iron inserted into the piece of rubber, and having the extremities connected with a thermo-galvanometer it was found that in this case the acoustic vibrations were converted into heat. Sheets of India-rubber therefore are among the best absorbers and destroyers of sound. A series of experiments was also made on the reflection of sound, to determine the materials least and those best adapted to this purpose. A résumé of these researches, having reference to the acoustic properties of public halls, was read before the American Association in August, 1856.

In 1865, as Chairman of the Committee of Experiments of the U. S. Light-house Board, Henry commenced an extended series of observations on the conduct and intensity of sound at a distance, under varying meteorological conditions. Well aware that for the practical purposes of giving increased security to navigation, the experiments of the laboratory were of little value, he undertook a number of experimental trips on board sailing vessels, and on steamers, in order to make his observations under the actual conditions of the required service. As many of his investigations required intelligent co-operation, and sometimes at the distances of many miles, he associated with him at different times, among members of the Light-house Establishment, Commodore Powell, Commodore Case, Admiral Trenchard, Commander Walker, Captain Upshur, General Poe, General Barnard, General Woodruff, Mr. Lederle, and other engineers of different Light-house Districts, and outside of the establishment, Dr. Welling and others.

At the outset of his experiments, he found that sound reflectors, which play so interesting a part in lecture-room exhibitions, were practically worthless (of whatever available dimensions) for the purpose of directing or concentrating powerful sounds to any considerable distance. At the distance of a mile or two a large steam whistle placed in the focus of a concave reflector 10 feet in diameter could be heard very nearly as well directly behind the reflector, as directly in front of it. In like manner the direction of bell-mouths and of trumpet-mouths, was found to be of comparatively little importance at a distance; showing the remarkable tendency to diffusion, especially with very loud sounds. Most of the observations made on ship-board were afterward repeated on land; and several weeks were occupied with these important researches.

"During this series of investigations an interesting fact was discovered, namely, a sound moving against the wind, inaudible to the ear on the deck of the schooner, was heard by ascending to the mast-head. This remarkable fact at first suggested the idea that

sound was more readily conveyed by the upper current of air than the lower." After citing observations by others apparently confirming the suggestion of some dominant influence in the upper wind, Henry adds: "The full significance however of this idea did not reveal itself to me until in searching the bibliography of sound, I found an account of the hypothesis of Professor Stokes in the Proceedings of the British Association for 1857,* in which the effect of an upper current in deflecting the wave of sound so as to throw it down upon the ear of the auditor, or directing it upward far above his head, is fully explained."† A rough attempt was made in the course of these observations (which were undertaken at the light-house near New Haven, Connecticut) to compare the velocity of the wind in the upper regions with that near the surface of the earth. "The only important result however was the fact that the velocity of the shadow of a cloud passing over the ground was much greater than that of the air at the surface, the velocity of the latter being determined approximately by running a given distance with such speed that a small flag was at rest along the side of its pole. While this velocity was not perhaps greater than six miles per hour, that of the shadow of the cloud was apparently equal to that of a horse at full speed."‡

In October, 1867, a series of observations was made at Sandy Hook (New Jersey) with various instruments. A sound reflector being employed, the distance at which the sand on the phonometer drum—carried in front, ceased to move was 51 yards, as compared with a distance of 40 yards, without the reflector. At a greater distance, with a more sensitive instrument, the ratio was very much diminished. Experiments were also made on the relative distances at which the trumpet affected sensibly the drum of the phonometer in different directions, giving as their result a limiting spheroid whose reach in the forward axis of the trumpet was about double that in the rear axis, and at right angles to the axis, was about a mean proportional between the two. With greater distances, these differences were evidently very much reduced, the radii becoming more equalized. In the summer of 1871, Henry made

* *Report Brit. Assoc.* vol. xxiv. 2d part, p. 27.

† *Report of Light House Board,* U. S. for 1874, p. 92.

‡ This difference has since been established by a number of independent observations. Mr. Glaisher from his balloon ascents in 1863–1865, ascertained that the upper currents of air are frequently five or six times more rapid than the surface currents. (*Travels in the Air*, p. 9.) Prof. Cleveland Abbe remarks: "From seven balloon ascensions made on July 4th, 1871, at different points in the United States, I have deduced the velocity of the upper currents as about four times that of the surface wind prevailing." (*Bulletin Philosoph. Soc.* Washington, Dec. 16, 1871, vol. i. p. 39.) And M. Peslin states in general terms: "It is certain according to all observations made both in mountains and in balloons, that the force of the wind increases considerably as we ascend in the atmosphere." (*Bulletin International de l'Observ. de Paris et de l'Observ. Phys. Cent. Montsouris*, July 7, 1872.)

investigations at different light-stations, on our western coast of California.

The very important observation that a sound could best be heard at an elevation when the wind is adverse (that is when it blows from the observer towards the acoustic signal,) and that after it had even been entirely lost to the ear in such case, it might be regained in full force by simply ascending to a suitable elevation, —admitted apparently but one explanation, namely that the line of successive impulse constituting a sound beam was deflected or bent upwards by the action of the opposing wind. If—as had already been shown to be the case sometimes, and as might therefore be expected generally,—the adverse wind were assumed to be a little stronger at the elevation than at the surface such a result would at once follow. "The explanation of this phenomenon as suggested by the hypothesis of Professor Stokes is founded on the fact that in the case of a deep current of air the lower stratum or that next the earth is more retarded by friction than the one immediately above, and this again than the one above it, and so on. The effect of this diminution of velocity as we descend toward the earth is in the case of sound moving with the current, to carry the upper part of the sound waves more rapidly forward than the lower parts, thus causing them to incline toward the earth, or in other words, to be thrown down upon the ear of the observer. When the sound is in a contrary direction to the current, an opposite effect is produced, the upper portion of the sound waves is more retarded than the lower, which advancing more rapidly in consequence, inclines the waves upward and directs them above the head of the observer."*

From several observed and reported cases where the sound of a fog-signal was exceptionally heard to a greater distance against the wind than toward the direction of the wind, Professor Henry for a while hesitated to give the hypothesis of Professor Stokes an unqualified acceptance; but forced as he was constantly to recur to it as the only plausible explanation of the ordinary influence of wind on the transmission of sound, he finally was able to satisfy himself that even the apparent exceptions to the rule were really in accord with it. Having more than once observed that when the upper current of air, as indicated by the course of the clouds, is in an opposite or different direction from the lower or sensible wind, the range of audibility is most affected and favored by the upper current, it was a natural induction to extend such a condition in imagination to other cases of abnormal behavior of sound. A large amount of subsequent labor and attention was devoted to the determination of this important question.

* *Report of Light House Board* for 1874, p. 106.

In 1872 it was observed from on board a steamer approaching Portland Head station in the harbor of Portland (Maine) that the fog-signal which had been distinctly heard through many miles, was lost to the ear when within two or three miles of the point, that it continued inaudible throughout the nearer distance of a mile or so, and that it was again heard as the station was neared. At Whitehead light station on a small rocky island about a mile and a half from the coast, (being some 65 miles northeast of Portland Head,) it was observed on board a steamer approaching the station during a thick fog, that the signal (a 10-inch steam whistle) though distinctly heard at the distance of six miles or more, and with increasing distinctness as the steamer advanced, was suddenly lost at about three miles, and was not recovered until within a quarter of a mile from the station; the wind at the time being approximately adverse to the sound. A six-inch steam whistle on board the steamer was meanwhile distinctly heard at the station during the whole time of inaudibility of the larger ten-inch whistle, which had also been sounded without any interruption. This remarkable phenomenon implied a compound flexure of the sound beams, and accorded with previous observations made at the same points by Gen. Duane the engineer in charge of the first and second Light-house Districts.

In 1873 observations were again made at Whitehead station, and at Cape Elizabeth light station, both on the coast of Massachusetts. At Whitehead the steam whistle was heard through a distance of 15 miles, with a light adverse wind. At Cape Elizabeth, with a stronger adverse wind, the siren was heard only about nine miles

In 1874, observations were made at Little Gull Island (off the coast of Connecticut); at Block Island, (off the coast of Rhode Island); and at Sandy Hook (New Jersey). At Little Gull Island the sound of a siren was heard against a moderate wind, only three and a half miles. At Block Island the siren was reported to have been heard under favoring conditions of wind through a distance of more than 25 miles. While it was frequently heard at Point Judith station, and the siren at the latter point was as frequently heard at Block Island, (the distance between the two points being 17 miles,) it was shown on comparison of records, that the two instruments had not been heard simultaneously; the wind when favorable to the one being unfavorable to the other.

At Sandy Hook, for the purpose of making simultaneous observations in different directions, three steamers (the tenders of different light-houses) were employed, with steam whistles specially adjusted to the same tone and power. The latter quality having been carefully tested by the phonometer, the three vessels steamed out abreast on trial; and their whistles sounding in regular succession "became inaudible all very nearly at the same moment." One of the vessels being then anchored at a distance from land,

the two others were directed in opposite courses, one with the wind, or eastward, the other against it, or westward. In 15 minutes the whistle of the former ceased to be heard, while that of the latter was very distinctly heard; the anemometer showing a wind of about six miles per hour. About noon the vessels changed positions, but the sound from the west continued audible for about three times the distance of that from the east, though the wind had declined to nearly a calm or to about half a mile per hour. In an hour and a half the wind had changed to "within two points of an exactly opposite direction, blowing from the indications of the anemometer at the rate of ten and a half miles per hour." The vessels once more departing, one with the wind, the other against it, the sound of the whistle coming against the wind was this time heard for the greater distance, contrary to expectation. On the following day a number of small balloons having been provided, a similar series of experiments to that of the preceding day was made; a station being selected at a greater distance from land. On the first trial, with a light wind from the west of about one and a quarter miles per hour as indicated by the anemometer, a balloon was set off which continued rising and moving eastward till lost to sight. Two of the vessels taking opposite courses as before, gave the sound in the direction of the wind about double the duration of that coming against the slight wind. The vessels then changed places in their opposite courses; the wind having subsided to a calm. "A balloon let off ascended vertically until it attained an elevation of about 1,000 feet, when turning east it followed the direction of the previous one. In this case the sound of the whistle coming from the east was heard somewhat longer than the opposite one. At the third trial made after noon, the wind had changed nearly one-third of the circle, its force being about five miles per hour. The vessels once more taking their courses with the wind and against it, "several balloons set off at this time were carried by the surface wind westwardly until nearly lost to sight, when they were observed to turn east, following the direction of the wind traced in the earlier observations." In this case the sound was heard with the wind very slightly farther than against it. It was thus shown that the upper current of wind had remained constant throughout the day, while the changing surface wind was apparently a land and sea breeze "due to the heating of the land as the day advanced:" and the varying behavior of the sound beams was easily explained by the varying differences of velocity in their wave fronts at different heights.

In 1875 Henry continued his observations at Block Island (R. I.) and at Little Gull Island (Conn.). The southern light-house on Block Island standing on the edge of a perpendicular cliff 152 feet above the sea level, and being itself 52 feet high (to its focal plane), this point was selected for making investigations on the

effect of altitude in modifying unfavorable conditions of audibility. Observers were accordingly stationed on the beach at the foot of the cliff, and also on the tower 200 feet above, to record simultaneously the duration of the whistle signals of two steamers proceeding in opposite directions toward the right and the left. The sound coming against the wind (of about seven miles per hour) continued audible at the upper station four times longer, (*i. e.*, for four times greater distance) than at the lower station. The sound coming with the wind, was unexpectedly heard at the lower station for a longer period than at the upper one. Another observation (with the wind about five miles per hour) gave for the sound against the wind, rather more than twice the distance of audibility at the upper station; and for the sound favored by the wind, a slightly greater distance at the top than at the bottom station. The next observation gave as before, with the adverse wind, the advantage of more than double the distance of audibility to the upper station; meanwhile one of the observers at the foot of the cliff, after the sound was entirely lost, managed by climbing to a ledge about 30 feet above the beach, to recover the signal quite distinctly, and to hear it for some time. The sound coming with the wind continued to be heard at both the higher and the lower stations for precisely the same time, giving on this occasion no advantage to either. Observations made on board the two steamers while moving in opposite directions, gave for the sound travelling with the wind a duration and distance more than five times that for the sound which came against the wind. Five similar experiments gave very similar results. The two vessels moving in opposite courses, each at right angles to the direction of the wind, gave a very close equality for the reciprocal durations of the sound. In the following month, similar observations were made at Little Gull Island, which were very accordant with those made at the former station. As a result of plotting the ranges of audibility in different directions from a given point, producing a series of circular figures (more or less distorted) of very different sizes, Henry was inclined to believe that the whole area of audition is less in high winds than in gentle winds. These investigations as their author well remarks,—"though simple in their conception have been difficult and laborious in their execution. To be of the greatest practical value they were required to be made on the ocean under the conditions in which the results are to be applied to the use of the mariner, and therefore they could only be conducted by means of steam vessels of sufficient power to withstand the force of rough seas, and at times when these vessels could be spared from other duty. They also required a number of intelligent assistants skilled in observation and faithful in recording results."*

* *Report of the Light-house Board* U. S. for 1875, p. 107.

In the summer of last year, 1877, with undiminished ardor, he continued his observations on sound; selecting this time Portland harbor, Monhegan Island, and Whitehead light station, on the coast of Maine. At the latter station, the abnormal phenomenon of a region of inaudibility near the fog-signal, and extending outward for two or three miles, (beyond which distance the signal is again very distinctly heard,) had for several years been frequently observed. This singular effect is noticed only in the case of a southerly wind when the vessel is approaching the signal from the same quarter, and consequently with the wind adverse to the direction of the sound beams, a condition of the wind which is the usual accompaniment of a fog. The observation showed this intermediate "belt of silence" to be well marked on board the steamer both on approaching the station and on receding from it by retracing the same line of travel. Meanwhile the intermittent signal whistle from the steamer was distinctly heard at the station on both the outward and homeward trips of the vessel, throughout its course. The next set of observations was made on the opposite side of the small island, by directing the course of the steamer northward; and in this case the shore signal was distinctly heard throughout the trip, while the signal from the vessel passed through the "belt of silence" to the observers at the station. The hypothesis of a local sound shadow of definite extent, is excluded by the simple fact that the regions traversed were entirely unobstructed, the two points of observation—movable and stationary—being constantly in view from each other when not obscured by fog. The hypothesis of a stationary belt of acoustic opacity is equally excluded by the uninterrupted transmission of sound through the critical region in one direction; and this too whichever order of observation be selected. So that in one of the cases the powerful whistle ten inches in diameter blown by a steam pressure of 60 pounds, failed utterly to make itself heard, while the sound from a much feebler whistle only six inches in diameter and blown by a steam pressure of 25 pounds, traversed with ease and fulness the very same space. The only hypothesis left therefore is that of diacoustic refraction; by which the sound beam from one origin is bent and lifted over the observer, while from an opposite origin the refraction is in a reversed direction; and such a quality in the moving air is referable to no other observed condition but that of its motion, that is to the influence of the wind. Observations were afterward made at Monhegan Island, on some of the more normal effects of the refraction of sound by differences of wave velocity, all fully confirming the supposition which had been so variously and critically subjected to examination.

The principal conclusions summed up in this last Report for 1877, are: 1st. The audibility of sound at a distance depends

primarily upon the pitch, the intensity, and the quantity of the sound: the most efficient pitch being neither a very high nor a very low one,—the intensity or loudness of sound resulting from the amplitude of the vibration, and the quantity of sound resulting from the mass of air simultaneously vibrating. 2nd. The external condition of widest transmission of sound through the air is that of stillness and perfect uniformity of density and temperature throughout. 3rd. The most serious disturbance of the audibility of sound at a distance, results from its refraction by the wind, which as a general rule moving more freely and rapidly above than near the earth, tends by this difference to lift the sound-beams upward when moving against the wind, and in a downward curve when moving with it. 4th. When the upper current of air is adverse to the lower or sensible wind, or whenever from any cause the wind below has a higher velocity than that above—in the same direction, the reverse phenomenon is observed of sound being heard to greater distances in opposition to the sensible wind than it is when in the direction of the surface wind. 5th. While suitable reflectors and trumpet cones are serviceable in giving prominent direction to sounds within moderate or ordinary distances, yet from the rapid diffusibility of the sound-beams, such appliances are worthless for distances beyond a mile or two. 6th. The siren has been frequently found to have its clearest penetration through a widely extended fog, and also through a thick snow-storm of large area. 7th. Intervening obstructions produce sound shadows of greater or less extent, which however at a distance but slightly enfeeble the sound owing to the lateral diffusion and closing in of the sound waves. 8th. The singular phenomenon of distinct audibility of sound to a distance with a limited intermediate region of inaudibility where no optical obstruction exists, is due sometimes to a diffusion of upper sound-beams which have not suffered the upward refraction; sometimes to the lateral refraction of sound-beams or to the lateral spread of sound from directions not affected by the upward refraction; and very frequently to a double curvature of the refracted sound-beams under an adverse lower wind, by reason of the wave fronts being less retarded by the lower or surface stratum of wind than by that a short distance above, and at still greater heights being again less retarded, and finally accelerated by the superior favoring wind.

These remarkable series of acoustic investigations undertaken after the observer had considerably exceeded his three score years, —perseveringly continued weeks at a time, and sometimes for more than a month,—extending through a period of twelve years, and pursued over a wide and extremely irregular range of sea-coast, and under great variety of both topographical and meteorological conditions — untiringly prosecuted by numberless sea

trips of 10, 15 and even 20 miles in single stretches, in calm, in sunshine, in storm, with every variety of disregarded exposure, form altogether a labor and a research—quite unequalled and unapproached by any similar ones on record. As a result of so great earnestness and thoroughness in the conduct of an enterprise of so great difficulty, Henry has advanced and enriched our knowledge by contributions to the science of acoustics unquestionably the most important and valuable of the century. By persistent cross-examination of the bewildering anomalies of sound propagation under wide diversities of locality and condition, he has succeeded in evolving order out of apparent chaos, —in reclaiming a new district, now subjected to the orderly reign of recognized law,—and in raising the plausible but long neglected hypothesis of Stokes into the domain of a verified and fully established theory. Only on the subject of the ocean echo had he failed to reach a solution which entirely satisfied his judgment;* and at the ripe age of four score years he had mapped out a further extension of his laborious search after truth, when his beneficent and all unselfish purposes were cut short by death.

With these great labors (a full demand upon the energies of youthful vigor) fittingly closed the life of one whose long career had been dedicated to the service of his race,—no less by the unrecorded incitations and encouragements of others to the prosecution of original research, than by his own earnest efforts on all convenient occasions to extend the boundaries of our knowledge. Nor is it permitted us to indulge in vain regrets that thirty years of such a life were seemingly so much withdrawn from his own chosen ministry at the altar of science, to be occupied so largely with the drudgery and the routine of merely administrative duties. True though it be, that talents adapted to such functions are very much more common and available than those which form the successful interrogator of Nature, who that knows by what exertions Smithson's wise endowment was rescued from the wasteful dissipation of heterogeneous local agencies and objects—by what heroic constancy, and through what ordeals of remonstrance and

* "The question, therefore, remains to be answered: what is the cause of the aerial echo? As I have stated, it must in some way be connected with the horizon. The only explanation which suggests itself to me at present is, that the spread of the sound which fills the whole atmosphere from the zenith to the horizon with sound-waves, may continue their curvilinear direction until they strike the surface of the water at such an angle and direction as to be reflected back to the ear of the observer. In this case the echo would be heard from a perfectly flat surface of water, and as different sound-rays would reach the water at different distances and from different azimuths, they would produce the prolonged character of the echo and its angular extent along the horizon. While we do not advance this hypothesis as a final solution of the question, we shall provisionally adopt it as a means of suggesting further experiments in regard to this perplexing question at another season." (*Report of Light House Board*, 1877, p. 70.)

misconception, of contumely and denunciation, the modest income of the fund (husbanded and increased by prudent management) was yearly more and more withdrawn from merely popular uses and interests, and more and more applied to its truest and highest purpose, the fostering of abstract research, the founding of a pharos for the future,—the "increasing and diffusing of knowledge among men,"—who that knows all this, can say that Henry was mistaken in his devotion, or that his ripest years were wasted in an unprofitable mission? But in addition to this vast work,—accomplished as probably no one of his scientific compeers would have had the fortitude and the indomitable persistence to carry through, his personal contributions to modern science (as has been shown) have in the meantime been neither few nor unimportant.

One remarkable circumstance relating to Henry's directorship of the Smithsonian publications (which have had so wide a distribution and influence)* must not be here passed over. Having himself amidst the absorbing occupations of his position conducted so valuable original investigations—on the strength of building materials,—on the best illuminants and their proper conditions,—and especially in his last great labor on the philosophy of sound, we should naturally expect to find them displayed in the "Smithsonian Contributions;"—where in interest and importance second to none contained in that extensive and admirable series, these memoirs would have found their fitting place, and have given honor to the collection. But as if to avoid all semblance of a personal motive in his resolute policy of administration, he published nothing for himself at the expense of the Smithsonian fund; his numerous original productions being given to the public through the channel of various official reports. And thus it has occurred that his writings scattered in the different directions which seemed to him at the time most suitable, with little thought of any special publicity or perpetuity, have largely failed to reach the audience which would most appreciate them. And many of his most valuable papers—never by himself collected—must be searched for in unsuggestive volumes of Agricultural or Light-House Board Reports.†

* "The number of copies of the Smithsonian Contributions distributed, is greater than that of the Transactions of any scientific or literary society; and therefore the Institution offers the best medium to be found for diffusing a knowledge of scientific discoveries." (*Smithsonian Report* for 1851, p. 202.)

† Many valuable communications made to the American Association, to the National Academy of Sciences, to the Washington Philosophical Society, and to other bodies, from rough notes, which their author was prevented from writing fairly out, by the unceasing pressure of his multitudinous official and public duties, have unfortunately been published only by title.

For him it seemed enough that what was once established, would not be willingly let die; that the medium or the occasion of communication was of comparatively little consequence, if but a new fact or principle were thrown into proper currency, and duly accepted as part of the world's wealth: and beyond all ordinary men he seemed to feel the insignificance of personal fame as compared with the infinite value of truth. For such a man the most appropriate monument would be a full collection of his writings, produced in a worthy and appropriate style of publication.

PERSONALITY AND CHARACTER.

Of Henry's personal appearance, it is sufficient to say, that his figure, above the medium height, was finely proportioned; that his mien and movement were dignified and imposing; and that on whatever occasion called upon to address an assembly,

> "With grave aspect he rose, and in his rising seemed
> A pillar of state: deep on his front engraven
> Deliberation sat, and public care."

His head and features were of massive mould; though from the perfect proportion of his form, not too conspicuously so. His expansive brow was crowned with an abundant flow of whitened hair; his lower face always smoothly shaven expressed a mingled gentleness and firmness; and his countenance of manly symmetry was in all its varying moods, a pleasant study of the mellowing, moulding impress of long years of generous feeling, and a worthy exponent of the fine and thoughtful spirit within: wearing in repose a certain pensive but benignant majesty, in the abstraction of study a semblance of constrained severity, in the relaxation of friendly intercourse a genial frank and winning grace of expression. Like his intimate personal friend Agassiz, he seemed to stand and to move among men as the very embodiment of unfailing vigorous health and physical strength, and only a year ago, he walked with as erect and elastic a carriage,—with as firm and sprightly a step, as any one here present.

It is difficult to attempt even a sketch of Henry's intellectual character, without allusion to his moral attributes;—so constantly did the latter dominate the former. It may be said that the most characteristic feature of his varied activities was earnestness, and this as usual was the offspring—as much of a moral as of a mental purpose.

His mind was eminently logical; and this rational power was exhibited in every department of his theoretical or his practical pursuits. He never showed or felt uneasiness at necessary deductive consequences, if the premises were well considered or appeared to be well founded. If presented with the problem of an untried

case,—while avowing the necessity of reserve in predicting results, he seemed to have an almost intuitive apprehension of the operation of natural law. If confronted with an unfamiliar phenomenon, whether in the experience of others, or in his own observations, his imagination was fertile in the suggestion of test conditions for eliminating varying influences. While few have ever held the function of hypothesis in higher estimation as an instrument of research, no one ever held hypothesis in more complete subjection.

As a lecturer and instructor, he was always most successful. Free from all self-consciousness,—without attempting oratorical display, his expositions—in simple, direct, and conversational language, were so lucid, satisfying, and convincing, that they enlisted from the start and secured to the close, the attentive interest of his auditors.

In his sympathy with the pursuits of the rising generation of physicists was ever manifested a disposition to frequent consultation and interchange of views with them; as if (aware of the usual tendency to mental ossification with advancing years,) he thus sought by familiar association to drink at the fountain of perennial youth. And surely no one was ever more successful in retaining ife's coveted greenness in age;—not more in the geniality of his affections and in his undimmed faith, hope, and charity, for mankind,—than in his intellectual freedom from undue prejudices, and in his readiness calmly to discuss or adopt new theories.

And this leads to the reflection that in the seeming contrasts of his nature were combined qualities which formed in him a resultant of character and of temperament as rare as admirable With this great mobility of aptitude and of circumspection, this adaptability of mental attitude, he yet possessed an unusual firmness of resolution. With a manly sturdiness of conviction he presented an unvarying equability of temper and of toleration; and with perfect candor as perfect a courtesy. With a characteristic dignity of figure of presence and of deportment, he preserved an entire freedom from any shade of arrogance. With a warm and active charity, he still displayed a shrewd perception of character; and while ever responsive to the appeals of real distress, his insight into human nature protected him from being often deceived by the wiles of the designing. Intolerant of charlatanism and imposture, he was capable of exhibiting a wonderful patience with the tedium of honest ignorance. Possessing in earlier life a natural quickness of temper, and always a high degree of native sensibility, his perfect self-control led the casual acquaintance to regard him as reserved and unimpressible. Of him it may be truly said in simple and oft-quoted words:

> "His life was gentle; and the elements
> So *mixed* in him, that Nature might stand up
> And say to all the world—This was a MAN!"

With all his broad humanity, he possessed but little of what is known as "humor." He could more heartily enjoy the ludicrous as drolly narrated by its appreciative victims, than when sarcastically recited at the expense of another. The sparkle of wit he fully appreciated provided it were free from coarseness and from personal satire. From the subordination of his sense of humor to his native instinct of sincerity, he had no approbation—or indeed tolerance of "practical jokes," holding that the shock to the feelings or to the confidence of the dupe, is far too high a price for the momentary hilarity enjoyed by the thoughtless at a farcical situation. Newspaper hoaxes—literary or scientific, in like manner received his stern reprobation, as uncompensated injuries to popular trust, and to the cause of popular enlightenment.

Strong in his unerring sense of justice and of right, he allowed no prospects of personal advantage to influence his judgment in action, in decision, or in opinion. He never availed himself of the opportunities offered by his position, of reaping gain from profitable suggestions or favorable awards: and he never willingly inflicted an injury even on the feelings of the humblest. This was characteristically shown in the pains taken to convince the judgment of those against whose visionary projects he was so often called upon to report in the public interests of the Smithsonian Institution, of the Light-house service, and of the General Government:—often expending an amount of valuable time and of patience which few so situated would have accorded, or could have well afforded. And yet on the other hand when himself the subject of injustice, misconstruction, or abuse, he never suffered himself to be provoked into a controversy;—as if holding life too serious, time too precious, to be wasted in mere disputation Least of all did he ever think of resorting to retaliatory conduct or to the expression of opprobrious sentiments. He calmly put aside disturbing elements, and seemed endowed with the power of excluding from his mental vision all irritating incidents. In that benignant breast there harbored no resentments.

To those who knew the man,—to those who have enjoyed the charm of his more intimate society, and felt the magnetism of his cheery presence, how poor and insufficient must appear these disjointed outlines of that mental, moral, and spiritual nature, which always and at every point was so much larger than it seemed.

Less than a year ago, (on the evening of November 24th, 1877,) he delivered in this place before this Society his annual address, shortly after his re-election as its President;—an address which as we beheld the remarkable fulness and freshness of the speaker's mental and bodily powers,—we little thought was in reality his valedictory. In it he concisely yet lucidly portrayed for the stimulation of more youthful physicists, the processes and the qualities necessary for success in original research;—the awakened attention

to "the seeds of great discoveries constantly floating around us," —the careful observation, the clear perception of the actual facts uncolored as much as possible by *a priori* conceptions or expectations,—the faculty of persevering watchfulness, and the judgment to eliminate (with all due caution) the conditions which are accidental,—the importance of a provisional hypothesis,—the conscientious and impartial testing of such by every expedient that ingenuity may suggest,—the lessons taught by failure,—the firm holding of the additional facts thus gleaned, though adverse and disappointing,—the diligent pondering, and the logical application of deductive consequences, to be again examined until as the reward of patient solicitation, the answer of nature is at last revealed.

"The investigator now feels amply rewarded for all his toil, and is conscious of the pleasure of the self-appreciation which flows from having been initiated into the secrets of nature, and allowed the place not merely of an humble worshipper in the vestibule of the temple of science, but an officiating priest at the altar. In this sketch which I have given of a successful investigation, it will be observed that several faculties of the mind are called into operation. First, the imagination, which calls forth the forms of things unseen and gives them a local habitation, must be active in presenting to the mind's eye a definite conception of the modes of operation of the forces in nature sufficient to produce the phenomena in question. Second, the logical power must be trained in order to deduce from the assumed premises the conclusions necessary to test the truth of the assumption in the form of an experiment, and again the ingenuity must be taxed to invent the experiment or to bring about the arrangement of apparatus adapted to test the conclusions. These faculties of mind may all be much improved and strengthened by practice. The most important requisite, however, to scientific investigations of this character, is a mind well stored with clear conceptions of scientific generalizations, and possessed of sagacity in tracing analogies and devising hypotheses. Without the use of hypotheses or antecedent probabilities, as a general rule no extended series of investigations can be made as to the approximate cause of casual phenomena. They require to be used however with great care, lest they become false guides which lead to error rather than to truth."* Who that listened could fail to see that the speaker was unconsciously giving us precious glimpses into his own experience?

Less than two weeks after this, he suffered at New York a temporary numbness in his hands, which he feared might threaten a paralysis; but a subsequent swelling of his feet and hands revealed

* *Bulletin Phil. Soc.* Washington, Nov. 24, 1877, vol. ii. p. 166.

to his physician the nature of his inward disease as a nephritis, which had been insidiously assailing him before it was suspected, and had doubtless been aggravated by his unremitting scientific labors continued as usual through his last summer vacation. Only a month before he died, he thus described the commencement of his malady: "After an almost uninterrupted period of excellent health for fifty years, I awoke on the 5th of December at my office in the Light-house Depot in Staten Island, finding my right hand in a paralytic condition. This was at first referred by the medical adviser to an affection of the brain, but as the paralysis subsided in a considerable degree in the course of two days, this conclusion was doubted, and on a thorough examination through the eye, and by means of auscultation, and chemical analysis, Dr. Weir Mitchell and Dr. J. J. Woodward pronounced the disease an affection of the kidneys."*

Aware that his illness was fatal, he yet felt lulled by that strange flattery of disease when unattended with a painful wasting, into the thought that he might probably survive the approaching warmer weather; and fully prepared for death, with the sense of life still strong within him, he planned what might yet be accomplished.

But with occasional alternations of more favorable symptoms, with the uræmia steadily increasing, his strength slowly declined: and as he lay at noon of the 13th of last May, [1878,] with growing difficulty of breathing—surrounded by loving and anguished hearts—his last feeble utterance was an inquiry which way the wind came. With intellect clear and unimpaired, calmly that pure and all unselfish spirit passed away—leaving a void none the less real, none the less felt, that the deceased had reached a good old age, and had worthily accomplished his allotted work.

Great as is the loss we have sustained of "guide, philosopher, and friend," we have yet the mournful satisfaction of reflecting that his influence, powerful as it always has been for good, still survives —in his works, his high example, and his unclouded memory;— that our community, our country, the world itself, has been benefited by his existence here; and that as time rolls on, its course will be marked by increasing circles of appreciation, reverence, and gratitude, for the teachings of his high and noble life.

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* Opening Address, written for the meeting of the National Academy of Sciences, April 16th, 1878. (*Proceed. Nat. Acad. Sci.*, vol. i. part 2, p. 127.)

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LIST OF SCIENTIFIC PAPERS; BY JOSEPH HENRY.

1825. On the production of cold by the rarefaction of Air: accompanied with Experiments. (Presented Mar. 2.) Abstract, *Trans. Albany Institute*, vol. i. part ii. p. 36.

1827. On some Modifications of the Electro-magnetic Apparatus. (Read Oct. 10.) *Trans. Albany Inst.* vol. i. pp. 22–24.

1829. Topographical Sketch of the State of New York; designed chiefly to show the General Elevations and Depressions of its Surface. (Read Oct. 28.) *Trans. Albany Inst.* vol. i. pp. 87–112.

1829. First Abstract of Meteorological Records of the State of New York, for 1828. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1829.

1829. On the Mean Temperature of Twenty-seven different Places in the State of New York, for 1828. (In conjunction with Dr. T. Romeyn Beck.) Brewster's *Edinburgh Jour. Science*, Oct. 1828, vol. i. pp. 249–259.

1830. Second Abstract of Meteorological Records of the State of New York for 1829. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1830.

1831. On the Application of the Principle of the Galvanic Multiplier to Electro-magnetic Apparatus, and also to the development of great Magnetic power in soft iron, with small Galvanic Elements. Silliman's *American Jour. Science*, Jan. 1831, vol. xix. pp. 400–408.

1831. Tabular Statement of the Latitudes, Longitudes, and Elevations, of 42 Meteorological Stations in New York. *Annual Report Regents of University* to Legislature N. Y. 1831.

1831. Third Abstract of Meteorological Records of State of New York for 1830. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1831.

1831. An Account of a large Electro-magnet, made for the Laboratory of Yale College. (In conjunction with Dr. Ten Eyck.) Silliman's *Am. Jour. Sci.* April, 1831, vol. xx. pp. 201–203.

1831. On a Reciprocating Motion produced by Magnetic attraction and repulsion. Silliman's *Am. Jour. Sci.* July, 1831, vol. xx. pp. 340–343. Sturgeon's *Annals of Electricity*, etc. vol. iii. pp. 430–432.

1832. On a Disturbance of the Earth's Magnetism in connection with the appearance of an Aurora as observed at Albany on the 19th of April, 1831. (Communicated to the Albany Institute, Jan. 26, 1832.) *Report of Regents of University*, to the Legislature of New York.—Albany, 1832. Silliman's *Am. Jour. Sci.* July, 1832, vol. xxii. pp. 143–155.

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1832. On the Production of Currents and Sparks of Electricity from Magnetism, Silliman's *Am. Jour. Sci.* July, 1832, vol. xxii. pp. 403–408.

1832. On the effect of a long and helical wire in increasing the intensity of a galvanic current from a single element. Silliman's *Am. Jour. Sci.* July, 1832, vol. xxii. p. 408. Becquerel's *Traité* expérimental de l'Électricité, etc. 1837, vol. v. pp. 231, 232.

1833. Fifth Abstract of Meteorological Records of the State of New York, for 1832. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1833.

1835. Contributions to Electricity and Magnetism. No. I. Description of a Galvanic Battery for producing Electricity of different intensities. (Read Jan. 14.) *Transactions Am. Philosoph. Society*, vol. v. pp. 217–222. Sturgeon's *Annals of Electricity*, etc. vol. i. pp. 277–281.

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1839. A Letter on two distinct kinds of dynamic Induction by a Galvanic current. (Read to Phil. Soc. Oct. 18.) *Proceedings Am. Phil. Soc.* vol. i. pp. 134–136.

1839. Observations of Meteors made Nov. 25, 1835, simultaneously at Princeton and at Philadelphia, for determining their difference of Longitude. (In conjunction with Professors A. D. Bache, S. Alexander, and J. P. Espy.) *Proceedings Am. Phil. Soc.* Dec. 21, vol. i. pp. 162, 163. Silliman's *Am. Jour. Sci.* Oct. 1840, vol. xxxix. pp. 372, 373.

1840. Contributions to Electricity and Magnetism. No. IV. On Electro-dynamic Induction. (Read June 19.) *Trans. Am. Phil. Soc.* vol. viii. pp. 1–18. Silliman's *Am. Jour. Sci.* April, 1841, vol. xli. pp. 117–152. Sturgeon's *Annals Electricity*, etc. vol. vii. pp. 21–56. *L. E. D. Phil. Mag.* June, 1841, vol. xviii. pp. 482–514. *Annales de Chim. et Phys.* Dec. 1841, 3d ser. vol. iii. pp. 407–436. Poggendorff's *Annal. der Phys. und Chem.* 1841, vol. liv. pp. 84–97.

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1846. Repetition of Faraday's Experiment on the Polarization of Liquids under the influence of a galvanic current. (Read Jan. 16.) *Proceedings Am. Phil. Soc.* vol. iv. pp. 229, 230.

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1855. Directions for Meteorological Observations. (In conjunction with Prof. A. Guyot.) *Smithsonian Report,* 1855, pp. 215–244.

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1855. On Green's Standard Barometer for the Sm. Institution. *Smithsonian Report,* 1855, pp. 251–258.

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1856. On Acoustics applied to Public Buildings. *Proceedings Am. Assoc.* Aug. 1856, pp. 119–135. *Smithsonian Report,* 1856, pp. 221–234. *Canadian Journal,* 1857, vol. ii. pp. 130–140.

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1878. Report on the Ventilation of the Hall of the House of Representatives. (Jan. 26.) 45th Cong. 2nd Sess. H. R. Report, No. 119, pp. 1–6.

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1878. Opening Address before National Academy of Sciences. (Read April 16.) *Proceedings Nat. Acad. Sci.*, vol. i. part 2, pp. 127, 128.

1878. Closing Address before National Academy of Sciences. (Read April 19.) *Proceedings Nat. Acad. Sci.*, vol. i. part 2, pp. 129, 130.

ADJOURNED MEETING. NOVEMBER 2, 1878.

Continued Memorial Meeting.

Vice-President WELLING in the Chair.

Mr. PETER PARKER read the following address commemorative of

JOSEPH HENRY.

MR. PRESIDENT AND MEMBERS OF THE
PHILOSOPHICAL SOCIETY OF WASHINGTON:

I desire to say a few words in memory of our lamented President, JOSEPH HENRY. Many have already pronounced his eulogy and set forth his rare talents and influence upon the world, and I need not, and could not well were I to attempt it, add to your appreciation of Professor HENRY, his life and character as a friend, scientist, his eminent services in the department of science, and as a Christian the highest type of man. For twenty years I have been intimately acquainted with Professor HENRY, and happily associated with him in many ways; for ten years as a Regent of the Smithsonian Institution. *I have never known a more excellent man.* His memory has been much on my mind since he left us, and I often find myself inquiring how he, and others like him, are occupied now? His connection with time is severed, but his existence continues. When I recall the names of BACHE, of PAGE, of AGASSIZ, and HENRY, and others of similar intellect and virtues, I detect myself asking the question, Are to them all consciousness and thought now suspended by separa-

tion from the body? I am reluctant to believe it. But, this I believe: *the Infinite Father's ways are right.*

It seems most providential that Professor HENRY had the opportunity and the strength to give in person his last words, a priceless legacy, to the National Academy of Sciences, and through that association to the civilized and scientific world. I refer to his sentiment that "*moral excellence is the highest dignity of man.*" The loftiest talent and highest attainments without this are deficient in that which in the judgment of wise men and of Infinite Wisdom, is of greatest worth. Was there ever a man from whom the sentiment could come with a better grace!

I have heard the opinion expressed, and do not think it extravagant, that the letter addressed by Professor HENRY to his valued friend, Joseph Patterson, emanating from such a mind, *such a man*, at the close of a protracted life of singular distinction, was worth a lifetime to produce. It has been read, probably, by millions, in various languages, and will be by future generations. The best tribute we as members of this Society can offer to the memory of our first President will be to emulate his virtues, and, far as practicable, to imitate his urbanity, his candor, nobleness of mind and heart, and his Christian character.

Professor HENRY was not only a man of science, a discoverer of nature's latent laws and forces, but a sincere believer in God, their author, and in His atoning Son. To quote his language, "We are conscious of having evil thoughts and tendencies that we cannot associate with a Divine Being, who is the director and governor of all, or call upon Him for mercy without the intercession of *One* who may affiliate himself with us."

I quote in conclusion from the prayer we offered at his funeral, to which we repeat our sincere *Amen*. [The lips that uttered them in one short month became silent in death, and the two remarkable men, Professors JOSEPH HENRY and CHARLES HODGE, closely united in life, were not long divided by death.]

"We thank Thee, O God, that JOSEPH HENRY was born, that Thou didst endow him with such rare gifts, intellectual, moral, and spiritual; that Thou didst spare him to a good old age, and enable him to accomplish so much for the increase of human knowledge, and for the good of his fellow men; and above all, that Thou didst hold him up before this whole nation as such a

conspicuous illustration of the truth that moral excellence is the highest dignity of man."

The following address by Mr. B. ALVORD was also read:—

JOSEPH HENRY.

On the 13th May, 1878, JOSEPH HENRY died in this city. No body of gentlemen have had such opportunities of watching the recent career of this distinguished man as the members of the Philosophical Society of Washington. The suddenness of the event is best realized by us when we recur to the clearness and firmness of his mind as evinced in contact with this Society the whole of last winter. His annual address on the 24th of November, 1877, was replete with the soundest advice to those who are entering on scientific investigations. We are struck with the freshness and elasticity of his mind and temper in his allusion to the method of scientific observation when he said: "There is a "story in a work entitled 'Evenings at Home,' which made an "indelible impression on my mind. It is entitled 'Eyes and No "Eyes,' and related to two boys who started on a walk during a "warm summer afternoon. On their return, one was fatigued, "dissatisfied, having seen nothing, encountered only dust and "heat; while the other was charmed with his walk, which had "been over the same ground, and gave a glowing account of the "objects which he had met with, and of the reflections which "were awakened by them."

Rarely does it happen that the labors of the scholar and student are continued with such undiminished powers up to the age of eighty. Besides his mental force and vigor, it is necessary to refer to the equipoise and judicial character of his temperament and organization. This fairness and careful avoidance of hasty judgments were important elements in his position as Director of the Smithsonian Institution. I had corresponded with him since 1850, for twenty-eight years, and the more we knew of him the more have all been impressed with his peculiar fitness for his task in that Institution.

It may be deemed unnecessary to recur to the amiable traits of his private character, and to his gentle and unostentatious demeanor; but in such a position these qualities form an important element in public character. Especially did this appear in

his exemption from dogmatism, so admirably portrayed in the words of General Sherman,* that he was free from the "arrogance of wisdom." This was aptly said of a man who, if any in this country, could claim the right to dogmatize.

The generous traits of his character are most vividly shown in the Report of the Special Committee of the Regents of the Smithsonian Institution (Annual Report, p. 88, Smith. Inst., published in 1858) presented by the late Prof. C. C. Felton on the matters between Samuel F. Morse and Prof. Henry. I refer especially to the letter of the Hon. Charles Mason, Commissioner of the Patent Office, dated March 31, 1856, stating that he was induced to grant the extension of Morse's patent for the telegraph in "accordance with the express recommendation of Professor Henry." It did not require the repeated dogma of the Supreme Court, that "a principle is not patentable; its practical application to some useful purpose constitutes the invention" (Brightly's Digest, Vol. I. p. 609; Vol. II. p. 275) to cause Professor HENRY not to stand in the way of patents in such cases. The bent of his whole life, the acmé of his ambition, the goal of his unflagging industry, was the discovery of scientific truths, the extension of the boundaries of human knowledge, and not the acquirement of wealth. Still it was but just that his brilliant discoveries in electro-magnetism, so essential in the invention of the telegraph, should be acknowledged.

His recent investigations and experiments in sound (as we of this Society have all seen) all bordered on the inventions of the telephone, the phonograph, and the microphone. One of his most valuable suggestions was the publication by the Royal Society of London of the Index of Scientific Papers, recently completed in nine volumes, the preface to which refers to its origin from the recommendation of Professor HENRY.

On the 15th of April last, a month before he died, I had a memorable conversation with him. As he had been denied the privilege, so long his habit and pleasure, of attending the meetings of this Society, I recounted for his amusement, at his request, a few of the items of one of our recent meetings, in which the fact that the motion of the inner satellite of Mars is more rapid than the motion of the planet, was the topic. And I referred to

* His Address at Princeton, June 19, 1878.

some of the theories which were in harmony with the Nebular Hypothesis. He emphasized his firm persuasion that however we may succeed in elucidating and confirming the Nebular Hypothesis, we do not in any way weaken the necessity of resorting to a belief and a faith in an Omnipotent First Cause, who chooses his own devices and methods to create and sustain the machinery of the Universe. Similar sentiments are forcibly presented in his remarkable letter of the 12th of April last to a friend in Philadelphia, printed in the appendix to the pamphlet "*In memoriam* of the funeral services."

He did not often dwell in public on these topics. No one ever lived more tolerant in the best meaning of the word than JOSEPH HENRY. And no one more clearly discerned the wisdom and necessity of keeping science and religion in their independent channels, so that neither should be obstructed. But he never held that for this purpose it was necessary to lop off, or stunt, or suppress the delicate tendrils, the emotions and intuitions which may lead the faithful student to such thoughts.

This association is termed the Philosophical Society. And Prof. HENRY was in the truest and most comprehensive sense a Philosopher. "*Philos,*" a lover; "*sophos,*" wise—a wise lover; or a lover of wisdom. It is true that the word is generally employed in reference either to pure and abstract science, or to phenomena and the logical deductions from those phenomena. And we, here in this Society, mainly dwell on the signal achievements of Prof. HENRY in those domains, and on the traits of his career and character which have thus made him, emphatically, in the view of the whole world, a great philosopher. But he did really attain his very highest position as a philosopher; he was in the widest sense a lover of wisdom, of truth, and of nature, with wonderful insight of its entire economy, when he went beyond the mere cultivation of his logical faculties, and also cherished a contemplation of his moral and spiritual being, and of *all* the ties and elements which environ man in the creation.

Remarks were made by Messrs. GILL, PARKER, WELLING, WOODWARD, and ELLIOTT, chiefly with regard to Professor HENRY on the Darwinian hypothesis and his willingness at an early period to receive it as a working hypothesis only.

Mr. NICHOLSON referred to experiments conducted by him in 1861 under the direction of Mr. HENRY on the six metre bar of the Coast Survey for determining its expansion by heat, and a series commenced, but not completed, for determining the effect of magnetism on metallic bars, showing a decided effect.

Mr. ELLIOTT spoke of the interest he manifested in the higher applications of mathematics especially to electricity, and the wish on his part to promote the branches of science, with which he did not profess to be completely familiar.

Messrs. ANTISELL, PARKER, and FARQUHAR spoke of the encouragement and assistance afforded by Mr. HENRY to young men in the prosecution of scientific researches.

APPENDIX.

V.

ON THE "PRODROMUS METHODI MAMMALIUM" OF STORR.

By THEODORE GILL.

(Read October, 1874.)

In the year 1780, Dr. Gottlieb Conrad Christian Storr, at that time prorector and a professor of the University of Tubingen, published a memoir on the classification of the mammals, under the title "*Prodromvs Methodi Mammalivm*,"[1] which has attained a certain degree of celebrity, and which at the same time is extremely rare. For nearly two years I endeavored in vain to obtain this publication, and applied to, or examined the catalogues of, the libraries of Congress and the Smithsonian Institution, and the chief libraries of Philadelphia, Boston, and New York, and also the Royal Society of London, the Zoological Society of London, and the Linnæan Society of London, and in the catalogues of none was the work mentioned. Through the Agent of the Smithsonian Institution in London, application was also made at the British Museum in order to have it copied, but nothing was known there of the publication. By a happy chance, however, a copy of the long desired work was recently procured by the indefatigable officer in charge of the library of the Surgeon-General's office, Dr. John S. Billings, in a lot of medical theses purchased by him. It is to be at once noted that this dissertation is a pamphlet of 43 pages and with four tables of classification annexed, and it thus corresponds with a publication noticed in several bibliographies under the title "Prodromvs Methodi Mammalivm et Avivm;" it is therefore probable that no other work with the title just mentioned exists. If conjecture may be hazarded, it is possible that the somewhat similar title of Illiger's work has been confounded with and carried over to the work of Storr. Storr's Prodromus is, however (with the limitations hereafter noted), exclusively confined to the mammals, the birds not being at all considered.

[1] Storr (Gottlieb Conrad Christian). Prodromvs Methodi Mammalivm.— — | Rectore Vniversitatis magnificentissimo | serenissimo atqve potentissimo | dvce ac domino | Carolo | dvce Wvrtembergiæ ac Tecciæ regnante, | rel. rel. | — | Ad institvendam | ex decreto gratiosæ facvltatis medicæ | pro legitime conseqvendo | doctoris medicinæ gradv | inavgvralem dispvtationem | propositvs | præside | Gottl. Conr. Christ. Storr | medicinæ doctore, hvivs, chemiæ et botanices | professore pvblico ordinario | vniversitatis H. T. pro-rectore, | respondente | Friderico Wolffer, | Bohnlandense. | — | *Tvbingæ, d. Jul. MDCCLXXX.* | — | Litteris Reissianis. [4to, 43 pp., 4 tables.]

Storr, after general remarks on methods and classification (which would contrast rather than compare with the views current among scientific taxonomists of the present day), discusses the classification in successively narrowed terms, of the groups including the mammals, dividing them as follows:—

(1) The animal kingdom into two *agmina;* one containing the Red-blooded Animals (RUBRISANGUIA), and the other the White-blooded animals.

(2) The *agmen* RUBRISANGUIA into two *acies:* the Warm-blooded and the Cold-blooded animals.

(3) The *acies* of Warm-blooded animals into two *classes:* MAMMALIA and AVES.

(4) The *class* MAMMALIA into three categories or *phalanges:* one (PEDATA) fitted for locomotion on the land; the second (PINNEPEDIA) for progression in the water rather than on the land; and the third (PINNATA) for progression exclusively in the water.[2]

(5) The *phalanx* PEDATA into two *cohortes:* distinguished, one by the presence of claws (*Cohors* UNGUICULATA), and the other by the presence of hoofs (*Cohors* UNGULATA).[3]

[2] "Sedes nimirum hisce animalibus non eadem omnibus facta est: maxima quidem eorum pars in terra potius vitam agit, quam in aquis, quibusdam tamen aquam magis, quam terram, habitantibus, aliquibus & aequori perpetuo commissis:

Triplici itaque *vitae generi* haud parcior respondit fabricæ mammalium varietas:

Terrena quidem artuum absolutissima fabrica eminent, qui, ab exortu liberi, commodeque trunco nexi, multiplici & artificiosissimo perficiendo motui aptati sunt;

Aquatili magis quam terrestri vitæ deditis minus in artubus habilitatis est, cum integumentis trunci ultra medietatem includantur, postremique præcipue artus ad imum ventrem revincti, qua parte demum solutiores prodeunt, apices porrigant ad incessum minus idoneos, ad remigandum contra magis adaptatos, complanatos, expansosque. Corpus præterea circa pectus ampliatum, hinc magis magisque extenuatum, sic quoque piscium quandum habitudinem induit.

* * * * * * * * * * *

Atque sic mammalium classis in *tres* secedit *phalanges.*" pp. 16, 17.

[3] Neque vero hactenus expositis momentis ita exhauriuntur omnes fabricæ eorum modi, omnisque horum ferax vitæ generis varietas, ut nullis porro locus sit ulterioribus modificationibus, quas vitæ genus tulerit subinde magis magisque definitum. Quin ipsa artuum conformatio novo crescentis varietatis mox exemplo est: Extremi pedatorum mammalium artus *corneis* quibusdam muniuntur *laminis*, quæ prouti in scutuli uncive incumbentis [*Unguiculata*], vel undequaque circumjecti calceoli [*Ungulata*] formentur modum, plus minus favent, obsunt partium earum expeditis, apto, validove motui. Respondet autem reliquo vitæ generi hæc quoque artuum fabrica. Neque adeo repudianda erat dudum adnotata *n*) unguiculatorum ungulatorumque animalium discrepantia, quæ quippe dignoscendis probe inferviat phalangis huius ambis cohortibus: *Prima sic unguiculatorum* cohors est, superimpositis, nec circumjectis corneis istis munimentis ad artus extimos armata; *Altera ungulatorum*, quæ velamento tali corneo obducta atque circumscriptos prodit artuum apices. p. 18.

(6) The *cohortes* into respectively three ordines,[4] distinguished by modifications of the teeth and digestive apparatus, viz.: (*a*) the cohors Unguiculata into the *ordines* (1) PRIMATES,[5] (2) ROSORES,[6] and (3) MUTICI;[7] (*b*) the cohors Ungulata into the *orders*[8] (1) JUMENTA,[9] (2) PECORA,[10] and (3) BELLUÆ.[11]

(7) The *Ordo* PRIMATES into two *missus*, distinguished by the presence or absence of hands: *Missus* (I.) MANUATI, and *Missus* (II.) EMANUATI.[12]

[4] Pedatorum mammalium utraque cohors, siquidem instrumentorum manducationis habeatur ratio, ad hoc momentum *triplicem varietatem* offert, unde *tot* utrique *ordines* subjiciuntur. p. 21.

[5] *Primus* nunc unguiculatorum *ordo* ea de cohorte animalia feligit, quæ manducationis apparatu instructissimo ac perfectissimo eminent. * * * * *Primatum* nomen primo ordini mammalium Linnæi iam auctoritate conciliatum huic meæ methodi mammalium primo æque ordini fervare nullus dubito, quum eodem primi loci titulo denominatione ista utar, majorem licet animalium numerum huic ordini adsciscı prolatæ meæ methodi leges præcipiant. Pleraque hujus ordinis animalia sunt carnivora, vel omnivora, aliquibus in usum quoque cedit victus vegetabilis, sed fructuum maxime & universe partium plantarum probe nutrientium. pp. 21–25.

[6] *Secundus* Vnguiculatorum *ordo* dentaria officina distinguitur ad rodendum tam exquisite adaptata, ut *rosorum* nomine hunc ordinem signare visum fuerit. pp. 26–27.

[7] *Tertius unguiculatorum ordo* manca dentarii apparatus conditione agnoscitur. p. 29.

[8] *Ungulatorum* animalium cohorti tres æque ordines subesse, præsignificatum fuit. p. 29.

[9] *Primus* nunc horum *ordinum* cum primo unguiculatorum ordine commune habet, quod dentium omni genere ambæ maxillæ polleant. * * * * *Iumentorum* nomen ordini, qui equum recipit, Linnæi olim *r*) inditum auctoritate, conservari etiamnum posse visum fuit. p. 29.

[10] *Pecorum ordo, ungulatorum alter*, satis quoque conspicuo signatus est naturæ sigillo, cum maxillarum & ventriculi fabrica, & pabuli conficiendi modus alius hujus ordinis animalibus, quam cuique alii, obtigerit: Unde & *ruminantium* nomine ab antiquissimus temporibus distingui consueverunt. p. 29.

[11] *Belluarum* indicandus *ordo* superest, *ungulatorum* animalium iis dicatus, quæ ad manducationis apparatum non tam perpetua circa genus dentium sibi aliud aliudque reservatum fabricæ conformitate, quam maxilloso ore, dentosisque maxillis agnoscuntur; Omnia enim hujus ordinis animalia prægrandes exhibent maxillas, dentibusque plurimum valentes, quum plerisque angulares dentes soleant in *arma* excrescere, extra os exserta, vel hiante saltem ore minitantia, feroque aspectu horrorem incutientia, in solitario autem Hydrochœri genere validorum dentium ingens numerus memorati dentosarum maxillarum characteris sustineat stabilitatem. Praterea & corpulentia insigni, immo, si a potiori parte fieri denominationem liceat, immani corporis mole eminent. Victu quidem vegetabili utuntur at copioso, & qui nutriendi facultate maxime præpolleat. p. 30.

[12] *Primatum* ordo mox peculiarem artuum difformitatem exhibet: Aliis enim in manus efformantur artus extimi, remoto & præ reliquis digitis

(8) The two *Missus* into severally three *sectiones*, distinguished by various characters.[13]

(9*a*) The *sections* of the *missus* MANUATI are differentiated by the modifications of the members, viz.: *Sectio* I. PALMARES, with the genus *Homo;*[14] *Sectio* II. PALMOPLANTARES, with the genera *Simia, Prosimia, Procebus, Tarsius, Lemur:*[15] *Sectio* III. PLANTARES, with the genera *Didelphis* and *Phalanger.*[16]

(9*b*) The *sections* of the *missus* EMANUATI are characterized (but erroneously) by the extent to which the feet are applied to the ground, and severally embrace the following genera,[17] viz.: *Sectio* I. (*Nocturni*) with nine genera, *Vespertilio, Sorex, Talpa, Erinaceus, Meles, Gulo, Mellivora, Ursus,* and *Nasua:*[18] *Sectio* II. (Unnamed), with two coetus; one (*Olaces*) with the genera *Procyon, Canis,* and *Hyæna;* the other (*Unci*) with *Felis* alone:[19] *Sectio* III. (*Verminei*), with the genera *Viverra, Mustela,* and *Lutra.*[20]

extensili pollice, aliis mannum subsidio orbatis, quibus ceteros ad digitos adpressus pollex est. Sic ordo hic in *missus binos* abit, *primum* quidem *manuatorum*, *emanuatorum* alterum. p. 31.

[13] *Manuatorum* denuo ad ipsum hoc momentum *trifaria* observatur diversitas, tot exprimenda *sectionibus*. p. 31.

[14] *Prima sectio palmares* habet, qui ad anteriores artus extimos, palmas dictos, manuati sunt, non item ad posteriores artus; Posteriorum artuum extremam partem palmis plantæ nomine opponi, inter Zoologos constat. p. 31.

[15] *Secunda sectio palmoplantares* recipit, quorum & palmæ manus referunt & plantæ. p. 31.

[16] *Plantares*, plantis quidem, neque vero palmis, manuati, *sectionem tertiam* stabiliunt. p. 31.

[17] *Emanuatorum* missus trifaria conditione metatarsorum in *tres* abit *sectiones:* p. 34.

[18] Prostratis & ad humum in incessu applicatis metatarsis *prima sectio* distinguitur, cui genera adscribo *Vespertilionis a*), *Soricis, Talpae, Erinacei, Melis, Gulonis, Mellivorae b*), *Ursi, Nasuae* (*c*); Quorum quidem animalium a vitæ genere haud alienum *nocturnorum* nomen videatur. pp. 34–35.

[19] *Sectio secunda* metacarpis atque metatarsis prædita subelongatis & arrectis, tellurem, cum incedit, non his, sed digitorum saltem apicibus, attingit: Peculiari unguium infra hanc sectionem occurente discrepantia *binæ* eius *scissiones* stabiliuntur; Quarum *prior* fixis & patentibus distincta unguibus, ad hoc momentum potiori unguiculatorum parti similem se exhibet, tria complectens genera, quæ hæc sunt: *Procyon d*), *Canis, Hyaena;* Odoratu plurimum valentes bestiæ *Olaces* dici possint.

Mobilibus & retractilibus distincta unguibus altera *scissio*, cui hamati, unci inde dicti, ungues sunt, *Felis* unum genus capit, quod, speciebus licet ditissimum, diuisionem tamen vix admittere videtur, quum in collatione horum animalium eiusmodi dotium, quæ ad docendam generis varietatem requiruntur, nulla adhuc innotuerit varietas. pp. 35–36.

[20] *Tertiae sectionis* declives in incessu atque accurtati tum metatarsi, tum præsertim metacarpi sunt. Cum elongati atque incurvati corporis & pedum

The other phalanges and orders have no other subdivisions than into genera, and are, under other names and with some rectifications, except the Pinnipeda, the same as the orders of Linnæus. The tables of Storr, copies of which are herewith given, will convey all necessary information respecting their contents.

The analysis of this classification will show that Storr erred to a greater extent than Linnæus had done in proceeding from the basis that because modifications had certain evident relations to the economy of the animal, they were therefore, and to the degree of their physiological influence, of importance in determining the affinities of those animals. As we will soon see, however, he greatly improved upon the genera of the systema mammalium by their limitation to species naturally and more closely allied.

Proceeding as the author thus did from a physiological basis, the major groups proposed by him cannot be compared strictly with those at present recognized by naturalists, but, so far as regards the subordination of recognized categories, the following names are as nearly synonymic as the nature of the facts will allow. For it must be further remembered that the term "genus" has undergone successive restrictions till now it corresponds with no named section of the older writers, their genera being rather equivalent—among the mammals at least—with the *families* of the moderns. With such qualifications, these are the approximate equivalents, viz:—

1. Agmen.	1. The two agmina are Vertebrates and Invertebrates.
2. Acies.	2. The acies are Warm-blooded and Cold-blooded Vertebrates.
3. Class.	3. Class.
4. Phalanx.	4. Subclass.
5. Cohors.	5. Superorder.
6. Ordo.	6. Order.
7. Missus.	7. } Suborder.
8. Sectio.	8. } Suborder.
9. Coetus.	9. } Suborder.
10. Genus.	10. Family.
	11. Subfamily.
	12. Genus.

Although the new genera were not characterized in distinctive diagnoses, many of the most familiar were first recognized and introduced into the system by our author. These were, however, only enumerated, along with the old genera, as the final divisions of the including groups, and all that was said respecting them was placed in the form of foot-notes. These foot-notes are here

opa abbreviatorum his animalibus ad perreptanda quæque locorum claustra plurimum obtigerit aptitudinis, *verminei* generis nomen positum olim fuit. *Tria* in hac sectione distinguuntur, *Viverrae*, *Mustelae*, *Lutrae genera;* Plura etiam deposcere videtur specierum adhuc cognitarum multiplex, nec tamen satis extricata, nec exhausta varietas.

reproduced so far as they relate to the newly named or newly modified genera.

The newly established genera were named respectively (1) *Prosimia* (adopted from Brisson and retained for the Lemurs);[21] (2) *Procebus* (established for Lemur catta, Linn.);[22] (3) *Tarsius* (universally accepted);[23] (4) *Phalanger;*[24] (5) *Meles* (not com-

[21] *t*) Prosimiæ nomen a Cel. Brisson introductum iis adhibeo Lemurem alias vulgatiori nomine signatis animalibus, quæ characterem generis revera sustinent, segregaturus ea, quæ dentium distent fabrica, in generum limitibus definiendis magni omnino facienda; Nec consanguineus huic generi Pennanti Lemur flavus videatur, quem alienum quoque udicasse modo laudatum Schreber (l. c. p. 146) moneo, ne forte solus, nimiove ductus systematis amore, ita sentire videar. Commodius hanc bestiam censuerim ad tertiam referri emanuatorum sectionem, quorum sic novum indicaverit cum manuatis vinculum; Ita vero generis peculiaris dignitatem sibi vindicaverit. p. 32.

[22] *u*) Procebi genus statuendum Lemur Catta Linn. postulare visus est, dentium quippe fabrica satis a prosimiis distinctus, & reliqua forma moribusque etiam ab iis non nihil recedens. Singulare animal, cujus plures nondum species noscantur, genere secerni, nec arcessitum, neque nimis artificiosum iis videbitur, qui porro circumspicientes nullibi vel generum, ordinumve, vel classium adeo, imo & regnorum in naturæ imperio tam exæquatum deprehendant modum, ut specierum quidam numerus ad genus statuendum necessarius habendus esset. Luculento satis argumento fuerit humanum genus in species nequa quam dirimendum, vicinum tamen polymorpho pithecino generi, nec homogeneus iumentorum ordo juxta pecorum ordinem generum feracissimum dignitate sua visus fuerit excidere, neque demum longe quidem angustior mammalium classis præ vermium insectorumve numerosa gente nulla habeatur. pp. 32–33.

[23] *x*) Vix alio in genere, quam eo, quod Lemuris signari nomine consuevit, tot animalia occurrunt, alienissima licet ingenio, scriptorum tamen arbitrio intrusa. Quod quidem novo mox specimine exemplum Tarsii confirmat (J. C. P. Erxleben Syst. Regni Anim. p. 71. Lemur Tarsius. Lips. 1777. 8.), animalis tarsis elongatis, unde nomen fortitum est, facile agnocendi, nec tamen ea unice distincti nota, quam pro generis discrimine docendo neutiquam sufficere, paulatina longitudinis plantarum in genere rosorum ordini fubiecto obvia progressio commonstrat (P. S. Pallas Novæ Species Quadrupedum e glirium ordine. Erl. 1778. 4. p. 88. & p. 276. Quo loco idem auctor Tarsium vocat Lemurem spectrum); Sed aliud & grave quidem discernendi Tarsii generis e dentium diversa fabrica accedit argumentum. Monendumque, dentes huius animalis, ex Oceani indici extremis insulis oriundi, Belgis nomine macassarico Podje dicti, exploratis pluribus speciminibus, paulo aliter, quam Jll. Daubenton enumerantur, describi Jll. Pallas (L. cit. p. 275.) "Primores nempe supra infraque tantum binos maiusculos, obtusos, hinc caninos primarios supra a primoribus, quibus vix longiores, remotos, infra magnos, primoribus approximatos, tum laniarios secundarios minores ubique binos, quorum supra anteriores minores." pp. 33–34.

[24] *z*) Phalanger ad Didelphidis genus, orientalis addito cognomine, aliis relatus, (P. S. Pallas Miscellan. Zoolog Hag. Com. 1766, 4. p. 59. Erxleben Syst. R. A. p. 79.) tum supra (p. 27 s.) memorata dentium conditione singulari, tum & fecundi plantæ digiti cum tertio coalitu prorsus peculiari distinctissimus, genus sistit manifeste satis discrepans, quin nonnihil anomalum. p. 34.

mented upon); (6) *Gulo* (from Klein and not noted); (7) *Mellivora* (Act. Holm. 1777, t. 4, f. 3: not otherwise referred to); (8) *Nasua;*[35] (9) *Procyon;*[36] (10) *Glis* (not of Erxleben, but em-

[35] *c*) Quod Viverris, nasuæ & naricæ cognomine adiecto, Linnæus maluit adscribere (Syst. nat. T. I. p. 64. 2. 3. Holm. 1766. 8), ursini generis vicinisæ restituendum animal, at genere tamen, ut & Melem & Gulonem, ab Urso distinguendum esse, tum ex pluribus aliis argumentis, tum maxime ex dentium pedumque manifesta ad generaliorem quidem modum cum ursino genere convenientia, eorumdem tamen momentorum ad specialorem modum sua singulis expensa discrepantia, legitime colligere mihi videor, nec rationes ceterarum dotium obesse juto. Folliculus certe putorius tum Nasuæ, tum Melis adstruendæ cum Viverris consanguinitati (*Blumenbach Handbuch der Naturgeschichte.* S. 96. 97. *Gött.* 1779. 8.) nullus sufficit, cum nec Viverris omnibus, nec solis competat (Affinitatum animalium Tab. Praes. J. Herrmanno. Resp. G. C. Würz. Argent. 1777 4). p. 35.

[36] *d*) Multiplice cum aliis comparatione hoc quidem animal obscuratum sæpius quam illustratum fuit: Tum Prosimiæ, tum Cani dotibus quibusdam simile indicat Jllustrissimus Comes de Buffon (Hist. nat. gen. & part. T. VIII. le Raton.), qui Melis quoque iunioris formam & staturam in eo deprehendit. Nec. Jll. Daubenton (ib.) dissentit, qui præterea vulpini rostri urget similitudinem. Meli adeo congener idem fecit animal Anatomiæ miscellaneæ auctor Major, qui folliculum ei putorium affinxit, ab aliis scriptoribus reiectum, denuo autem & geminum quidem propositum auctore Erxleben (l. c. p. 165), cui visum glandulas Daubentonio adnotatas, iuxta intestinum rectum sitas, inque hoc, intra anum amplo utrinque ostio hiantes in folliculos putorios transfigurare. Ad Ursum, nomine lotoris addito Linnæus retulit (Syst. Nat. T. I. p. 70.) ab ipso observatum descriptumque animal (*Der Königl. Schwed. Akad. d. Wiss. Abhandl. aus d. J.* 1746. *B.* 9. *S.* 300 *ff. Hamb.* 1753. fed gravia argumenta opposuit & ipse avtopta Roloff (Mém. de l'Acad. Roi. *Ann.* 1756. *Berl.* 1758. *p.* 149. *ss.*) Nasuæ adfine idem animal in patria haberi, commune utrique Coati nomen (J. de Laët novus orbis Lugd. Bat. 1633, fol. p. 553. G. Marggravii de Liebstatt. Hist. rer. nat Brasiliæ Lugd. Bat. 1648. fol.) indicat. Discrimen data opera exposuit laudatus Buffon (l. c.), vixque alio, quam topographi, errore accidere potuit, ut Linnæana huius bestiæ descriptio ad Nasuæ historiam in opere Buffoniano referretur. Cani, eiusve speciei, vulpi, analogum domestica quoque compellatio (G. Charleton exercitationes. Oxon. 1677. fol. p. 15. M. Catesby, nat. hist. of Carolina &c. app. p. 29. Lond. 173. fol.) pronuntiat, nec valde abludit alia etiamnum batvis Indiæ occidentalis colonis familiaris, qui Boschho en den vocant, teste Nob. Respondente, cui ibi commemorato notissimum animal fuit. Sed denominationum, quas aliquando ineptissimus usus fovet, auctoritatem minus tutam inter alia & Felis nomen Americanæ eidem animali inditum commonstrat (J. D. Meyers, *Vorstellung allerhand Thiere und ihrer Scelete Nürnb.* 1748, *fol. T. III. t.* 18.). Ex ipsa contemplatione naturæ huius animalis manifesta eius cum olacibus necessitudo abunde elucescere videtur; Nec sola argumento fuerit, qua plurimum valet, nasi sagacitas (*Schwed. Abhandl. a. a. st.*), quum & reliquarum partium ingeniique congruentiæ sat luculenta præstent documenta. Ceterum & idem animal olacum cum nocturnis adstruit viciniam, cum more quidem reliquorum sui cœtus animalium subelongatis & arrectis metatarsis gaudeat, nec nisi digitis tellurem, cum incedit, feriat, at plantis simul ita fabrefactis polleat, ut gradum sistens, renitensve adpressis firmiter ad humum talis potentius se sustinere valeat. pp. 35–36.

bracing the Myoxi and other forms);[27] (11) *Lagomys* (an unnatural and undefined combination of forms with squat bodies, but typified by species of Arctomys);[28] (12) *Procavia* (equivalent to *Hyrax* of Herrmann);[29] and (13) *Rosmarus*.[30]

New names were also conferred on genera retained with old limits because they did not come up to the ideas of Storr in respect to purity or aptness. These were, (1) *Cataphractus* (= *Dasypus*, Linn.); (2) *Pholidotus* (= *Manis*, Linn.); (3) *Aries* (= *Ovis*, Linn.);[31] (4) *Taurus* (= *Bos*, Linn.); and (5) *Diodon* (= *Monodon*, Linn.).[32]

The elimination of the species necessarily connected new ideas with the old genera from which they were rejected, and thus the following were modified, but by implication rather than by distinct diagnoses: (1) *Simia* (by the rejection of *Prosimia*, *Procebus*, and *Tarsius*); (2) *Ursus* (by the exclusion of *Meles*, *Gulo*, and *Procyon*); (3) *Canis* (*Hyæna* being excluded); (4) *Viverra* (by the elimination of the *Nasuæ*); (5) *Mustela* (by the removal of *Gulo*); (6) *Mus* (by the rejection of his "*Glires*" and "*Lagomyes*").[33]

The other genera were retained from previous authors, viz.:

[27] *h*) Glirium species eodem animo affero, nominibus potissimum Ill. Pallas usus, qui singula hæc animalia ad muris genus revocat: M. tamaricinus. M. longipes. M. cafer. M. sagitta. M. jaculus. M. nitedula. M. avellanarius. M. glis. p. 39.

[28] *i*) Sequuntur in eundem finem nomina specierum, laudato Pallas pariter ad mures tractarum, quæ mihi genus constituunt, Logomys, neo Arctomys dictum, nam Lepori aptius, quam Urso, comparari posse videantur. Dicendæ species, nominibus Ill. Pallas æque adhibitis, hæ sunt: M. arenarius. M. songarus. M. furunculus. M. cricetus. M. accedula. M. phæus. M. lagurus. M. gregalis. M. socialis. M. œconomus. M. rutilus. M. glareolus Schreberi. M. monax. M. marmota. M. empetra. M. arctomys. M. citillus. M. lemmus. M. torquatus. M. hudsonius. M. talpinus. M. capensis. M. aspalax. M. typolus. p. 39.

[29] *k*) Procaviæ genus africanum animal constituit, ut patria, sic dotibus quamplurimis a Caviæ genere distinctum, Cavia capensis Ill. Pallas dictum (Spicileg. Zoog. fascic. II. pag. 16, ss. Berol. 1767. 4.). p. 40.

[30] *q*) Rosmari binos in superiori maxilla primores detexit Ill. Schreber (*Säugthiere. II. Abtheilung* S. 260.). p. 41.

[31] The genera (1) *Ovis* and (2) *Bos* were also renamed independently, many years afterwards, by Rafinesque in accordance with the same principles (1) *Aries* and (2) *Taurus*.

[32] *t*) Vulgari circa huius animalis fabricam errori nimium favere Monodontis nomen videatur. p. 42.

[33] *g*) Murini generis limites qui mihi statuantur, expeditissime specierum brevis recensio explanabit; Sunt hæ: M. Castoreus (Castor Zibethicus Linn.). M. amphibius. M. arualis. M. agrarius. M. saxatilis. M. alliarius. M. minutus. M. betulinus. M. vagus. M. striatus. M. rattus. M. musculus. M. sylvaticus. M. decumanus. M. pylorides. M. caraco M. soricinus Hermanni. p. 39.

(*a*) from **Linnæus**, the genera (1) *Homo;* (2) *Didelphis;* (3) *Vespertilio;* (4) *Sorex;* (5) *Talpa;* (6) *Erinaceus;* (7) *Felis;* (8) *Lutra;* (9) *Hystrix;* (10) *Castor;* (11) *Sciurus;* (12) *Lepus;* (13) *Bradypus;* (14) *Myrmecophaga;* (15) *Equus;* (16) *Camelus;* (17) *Cervus;* (18) *Moschus;* (19) *Sus;* (20) *Rhinoceros;* (21) *Hippopotamus;* (22) *Phoca;* (23) *Trichechus* (= Manatus of later authors);[34] (24) *Delphinus*; (25) *Physeter;* and (26) *Balaena:* (*b*) from Erxleben, the genus (1) *Hydrochœrus:* (*c*) from Leske, *Lemur* (= Galeopithecus, Pallas, and later writers):[35] from Brisson (who was not a binomial writer) the genera (1) *Prosimia;* and (2) *Giraffa;* and (*d*) from Steller, *Manatus* in a modified form.[36]

The punctuation, capitalization, and orthography of the original are retained in the excerpts here given. The letters in italics (*c*, etc.) before the notes are also repeated from the original footnotes. The words included in brackets [ ] in the tables are inserted by the present writer in explanation of the text.

[34] *r*) Obscurum animal Dugong [*Trichechus*] dictum, oppido a praecedente genere [*Rosmarus*] distinctum & nomine distinguendum visum fuit. p. 41.

[35] *y*) Lemurum nomen, in agilissima adhuc memorata animalcula nullo modo quadrans, aptius servari animali visum fuit, Lemuris volantis nomine auctoribus plerisque venienti, peculiaris autem sibi vindicanti generis locum, cuius characterem paucis at nervose reddidit Cel. Leske (*Anfangsgründe der Naturgeschichte. I. Th. p.* 121. *Leipz.* 1779. 8). p. 34.

[36] *s*) Stellero memoratum animal Manati quidem nomine signatum (Nov. Comm. ac. sc. Petropol. T. II. pag. 302), ex descriptione potius ad pinnatorum relegaverim phalangem. p. 41.

TABVLA GENERALIOR.

Imperii Natvræ.

Regni Organici.

Reipvblica Animalivm.

Agminis Rvbrisangvivm.

Acies Calidorum.

Classis I. Mammalivm.

Phalanx I. Pedatorum. — Phalanx II. Pinnipedum. — Phalanx III. Pinnatorum.

Cohors I. Vnguiculatorum. — Cohors II. Vngulatorum.

Ordo I. Primates. — Ordo II. Rosores. — Ordo III. Mutici.

Ordo I. Jumenta. — Ordo II. Pecora. — Ordo III. Belluæ.

[PHALANX I. PEDATA.]

[COHORS I.—UNGUICULATA.]

TABULA SPECIALIOR A.

§ Sectio I. . . . Homo [L.]
§ [Palmares. 31.]
§ Simia [L.]
§ Prosimia [Brisson.]
§ Missus I. ∽ Sectio II. . . . Procebus [Storr.]
§ Manuati. [Palmoplantares.32.] Tarsius [Storr.]
§ § Lemur [Leske.]
§ §
§ § Didelphis [L.]
§ § Sectio III. . . Phalanger [Storr.]
Mammalium [Plantares. 33.]
Pedatorum
Vnguiculatorum Vespertilio [L.]
Ordo I. Sorex [L.]
Primates. Talpa [L.]
§ Erinaceus [L.]
§ Sectio I. . . Meles [Storr.]
§ § [Nocturni. 35.] Gulo [Storr.]
§ § Mellivora [Storr.]
§ § Ursus [L.]
§ § § Coetus I. Nasua [Storr.]
§ Missus II. § [Olaces. 35.] Procyon [Storr.]
Emanuati. ∽ Sectio II. . . . Canis [L.]
§ § Hyæna [Storr.]
§ §
§ Coetus II. Felis [L.]
[Unci. 36.]
§ Viverra [L.]
§ Sectio III . . Mustela [L.]
[Verminei. 37.] Lutra [L.]

TABVLA SPECIALIOR B.

Mammalium Pedatorum Vnguiculatorum	Ordo II. Rosores. . .	Hystrix [L.] Castor [L.) Mus [L.] Glis [Storr.] Sciurus [L.] Lagomys [Storr.] Cauia [Klein.] Procauia [Storr.] Lepus [L.]
	Ordo III. Mutici. . . .	Bradypus [L.] Cataphractus [Storr.] Pholidotus [Storr.] Myrmecophaga [L.]

TABVLA SPECIALIOR C.

[COHORS II.—UNGULATA.]

Mammalium Pedatorum Vngulatorum	Ordo I. Iumenta.	Equus [L.]
	Ordo II. Pecora	Camelus [L.] Giraffa [Brisson.] Aries [Storr.] Antilope [L.] Taurus [Storr.] Ceruus [L.] Moschus [L.]
	Ordo III. Belluæ	Sus [L.] Hydrochœrus [Erxl.] Rhinoceros [L.] Elephas [L.] Hippopotamus [L.]

[PHALANX II. PINNIPEDA.]

Mammalia Pinnipeda	Phoca [L.] Rosmarus [Schreber.] Trichecus [L.] Manatus [Steller ?]

[PHLANX III. PINNATA.]

Mammalia Pinnata	Delphinus [L.] Diodon [Storr.] Physeter [L.] Balæna [L.]

VI.

ON THE NUMBER OF WORDS USED IN SPEAKING AND WRITING.

By EDWARD S. HOLDEN.

(Read January 30, 1875.)

The question which I have proposed to myself is to determine the size of my own vocabulary; that is, to fix approximately the number of words of which I may be supposed to be master. To do this *accurately*, two things are necessary: first, the collected works of an author, and second, the time necessary to form a complete concordance to these. In my own case neither of these prerequisites is fulfilled, and indeed, the object of the attempt is not to fix with absolute certainty the number in question, but simply to get an approximate solution, and if possible to determine the limit of error in the result. I approached the subject, as almost every one will do, with the impression that this vocabulary was very small. The only basis for this opinion that I know of, is a statement of Marsh that an intelligent man will use in speaking and writing less than 10,000 words. My impression was, that this number was too small, and it was to determine how much too small that I undertook the research.

For my purpose I define a *word* to be a symbol printed in capital letters in Webster's Dictionary, edition of 1852.

In turning over the leaves of a dictionary one meets with three classes of words: 1st, those which one is certain truly belong to him and are constantly used in writing and speech; 2d, those which one might use in writing or very formal conversation, but which it requires a moment's consideration to determine to include or not to include in one's vocabulary; and 3d, those rare or extraordinary words which one unhesitatingly rejects. It is to be noted, however, that technical words are not all in this last class, although a large part of this class is composed of them. For example, the vocabulary of the geologist contains many technical words which I never use in writing or speech, and this is true of other specialists; so that the 3d class of words mentioned above would not include the same words, by any means, for different members of this Society.

Literary men, however, would probably be nearly unanimous in their selection of this third class. Perhaps, here is the place to say that we ought to expect that the vocabulary of a literary man of even the highest class, like Thackeray, would be smaller than that of Huxley, for example. In counting the number of words in the dictionary which are properly to be included as in habitual use, one's natural tendency is to include too many of the 2d class spoken of, that is, too many words whose meaning is perfectly well understood, which would be intelligible if met with in reading, and which yet might not be used i a lifetime.

I have sedulously endeavored to avoid this tendency; and, indeed, I have gone over many of the pages previously examined, finding not more than one *per cent.* of words wrongly marked as my own.

I will give an account of my method of proceeding, asking attention to its details. As the basis of the inquiry I chose the unillustrated edition of Webster's Dictionary of 1852; unillustrated, because the average number of words to a page could be more easily had, as the page was not broken up by cuts. This edition contains 1281 pages of defined words. I first proceeded to find the relative frequency of the various letters of the alphabet as the *initial* letters of words. This has of course been often done, but I do not know where to refer to it. The number of pages devoted to each letter in my edition is as below:—

A 90.6; B 69.6; C 136.0; D 77.8; E 55.9; F 58.7; G 38.3; H 44.1; I 56.3; J 8.2; K 6.7; L 40.2; M 58.5; N 19.6; O 27.6; P 106.1; Q 8.0; R 69.7; S 150.5; T 64.3; U 35.6; V 20.6; W 33.0; X 0.4; Y 2.9; Z 1.6. The tenths were estimated. The order of frequency as initial letters is then:—

1, S; 2, C; 3, P; 4, A; 5, D; 6, R; 7, B; 8, T; 9, F; 10, M; 11, I; 12, E; 13, H; 14, L; 15, G; 16, U; 17, W; 18, O; 19, V; 20, N; 21, J; 22, Q; 23, K; 24, Y; 25, Z; 26, X. The relative frequency can be deduced from the numbers first given, and the further consideration that there were 1281 pages of words in this dictionary.

My next step was to find the average number of words to a page: to this end I counted—

10 pages in S and found	731 words
10 " " C " "	655 "
10 " " P " "	763 "
1 " " K " "	91 "
1 " " Y " "	59 "
1 " " Z " "	84 "
33 " containing	2383 "
1 page averages	72.2 words
The book contains	92,488 words.

I then proceeded to count the words that I use in writing or speaking. This was done rapidly, yet, as I have said, it was endeavored to keep out all unusual words except such as I felt that I had an undoubted right to retain.

Each word which in the text is printed in capitals was counted once for itself, but each of its meanings was *not* counted: except that a verb whether transitive or intransitive was only once counted. The counting was not done on a uniform plan; and I will give each result as it was obtained, in order that a verification can be had, if desired.

I first counted in A, S, C, P the four letters most frequently used, then in E, H, L and others about the mean, and then in K, Y, Z those which occur rarest as initials.

I. Letter.		II. Pages of Book.	III. No. of Pages.	IV. Total Words.	V. Words used.
4	A	pp. 1 to 16.6	16.6*	1199	500
1	S	" 974 " 979	...	455	200
2	C	" 162 " 166	...	300	86
3	P	" 790 " 793	...	300	107
9	F	" 462 " 463	2*	144	31
10	M	" 716 " 717	2*	145	51
11	I	" 590 " 591	2*	144	44
12	E	" 380 " 384	4*	289	115
13	H	" 550 " 554	4*	289	100
14	L	" 666 " 670	4*	289	110
15	G	" 492 " 493	2*	145	53
16	U	" 1210 " 1211	2*	144	37
17	W	" 1248 " 1249	2*	145	29
23	K	" 638 " 632	4*	289	102
24	Y	" 1278 " ...	...	59	20
25	Z	" 1281 " ...	...	84	14
			Total,	4420	1599

N. B. The numbers prefixed to the letters in the first column denote the order of frequency of those letters as initials.

This would give 33,456 words in my vocabulary, and this seems too great. I cannot see, however, how my process is not a fair one. One point where I may be in fault, may be in the determination of the total number of words included. At the beginning and at the end of each letter of the alphabet there is a blank space, which I have estimated can hardly be more than 15–100 of a page on the average, or 4 or at most 5 pages in all. This space has been counted as full, and hence we ought to subtract from 92,488 = the total number of words, about 300 on this account. Dr. Webster states in his preface to the first edition, that it contains between 70,000 and 80,000 words; and in the preface to the edition of 1840 it is stated that "several thousand" words have been added. Probably there are at least 90,000 words included: which would reduce the number of "words used," slightly. The entire number of words in the latest editions of Webster's Dictionary is 110,000, and I am convinced that the error in the concluded total number of words is not great.

It then becomes important to scrutinize carefully the other term of the ratio, *i. e.*, the number of words marked as used. As I stated, on examining several pages of words which I marked as *used*, I found an error of about one *per cent.*

Since my first scrutiny I have asked my friend Mr. Farquhar,

* The numbers in column IV, opposite numbers in column III, distinguished with asterisks, were derived by multiplying such discriminated numbers by 72.2 = the average number of words on a page.

Assistant Librarian in the Patent Office, to count the number of words which he would use on 8 of the pages previously counted by me. He has done so, and on pp. 550 to 554 in H, where I find 100 he has marked 142; and on pp. 380 to 384, where I find 115, he finds 131. This counting and selection was done in the most deliberate and critical way, and the excess of his numbers over mine shows not only that his vocabulary is larger than mine, but it further shows that my estimate was fairly made; and this is a point I am very glad to have so clearly established.

I have a letter from the eminent Prof. Whitney, of Yale College, on this subject, portions of which I quote: "I do not see that your method is not one which should yield a tolerably accurate result, nor am I disposed seriously to question the accuracy of the result you have reached." Prof. Whitney refers to Marsh, Lectures on the English Language, p. 181–2, who says that one person *may* be able to wield 50,000 of the 100,000 English words, but that "few writers or speakers use as many as 10,000; ordinary persons of fair intelligence not above 3000 or 4000."

Since the receipt of Prof. Whitney's note I have determined from Mrs. Mary Cowden Clarke's "Complete Concordance to Shakspere" the number of words in his vocabulary. Here, however, we have incomplete data, as "all nouns and verbs spelled alike are placed under the same heading."

I find in—

A	pp.	2	and	3	86	words.	F	pp.	258	and	259	27	words.
B	"	38	"	39	87	"	G	"	296	"	297	25	"
C	"	136	"	137	83	"	H	"	338	"	339	11	"
D	"	178	"	179	51	"	I	"	386	"	387	134	"
E	"	218	"	219	68	"	J	"	398	"	399	57	"

Counting also the words on the 35 pages, 1, 25, 50, 75, 100, 125, ... 825, 850, I find 926 words.

Therefore 55 pages have 1555 words, or 1 page has 28.3 words.

There are 859.5 pages of such words, and hence Shakspere's vocabulary (with the important omission of all verbs which are spelled like nouns) contained over 24,000 words.

A complete "Concordance to the Poems of Milton" has been published by Mr. Charles Dexter Cleveland, and I find that on 5 pages of this work there are 562 words, or 112.4 words to a page. The number of words to each page is quite uniform, so that five pages give a sufficiently accurate determination. The results from each page below will show this.

p. 600	108	words.
p. 601	116	"
p. 602	113	"
p. 603	114	"
p. 604	111	"
	562	" or average number of words to a page 112.4.

There are 154.6 of such pages, and hence Milton, in his poems alone, uses 17,377 words. His prose would yield a much larger number, as any one who is acquainted with it will at once admit.

I have likewise examined Cruden's "Concordance to the English Bible," in the same manner; there are 705.4 pages of words, exclusive of proper names.

The number of words to a page is somewhat hard to estimate, but this was done with great care, as below :—

pp. 524–563 = 10	pages contained	42	words.
pp. 602–611 = 10	" "	107	"
pp. 672–681 = 10	" "	119	"
pp. 328–337 = 10	" "	129	"
pp. 446–455 = 10	" "	114	"
50		511	"
Therefore 1	page contains	102.2	"

Hence there are 7209 words in the English Bible, exclusive of proper names.

I have likewise treated the "Dictionary of the Anglo-Saxon Language" by the Rev. G. Bosworth, LL.D., etc., in the same manner. This Dictionary was compiled from the Anglo-Saxon Chronicle, and therefore contains only words actually used in written speech. There are but a few which were not in full use before A. D. 1100.

In the English Appendix to this work there are 93.8 pages of English words, each of which has its analogue in Anglo-Saxon.

On p. 524 I find	121	words.
" p. 525 "	131	"
" p. 526 "	125	"
" p. 527 "	131	"
4 pages contain	508	"
1 page contains	127.0	"

Hence the Anglo-Saxon vocabulary was 11,913 words; it must be remembered that this is not strictly a dictionary, but rather a concordance to the Anglo-Saxon Chronicle.

In Mr. John Camden Hotten's "Dictionary of Slang" I find 10,000 words which are, or have been, used in a cant way. As this is a dictionary, and fortunately not a concordance, it deserves only this passing mention.

An examination of some of the dictionaries of the dialects of the various shires of England would be interesting, but it requires more time than I can give to it.

RECAPITULATION.

I. I find among all intelligent people an impression of this kind: a child uses less than 1000 words, an ordinary man uses from 3000 to 4000, an accomplished writer about 10,000.

This rests, so far as I can determine, upon the statement of Marsh, already quoted.

II. We have seen that Shakspere has over 24,000, and that Milton in his poems has over 17,000.

The Anglo-Saxon Chronicle contains about 12,000 words, and the English Bible, which is treating of quite special subjects, contains over 7000 words.

III. The whole number of words in Worcester's Dictionary is 104,000; in Webster's last edition 110,000 (these numbers are approximate). Many of these, in fact most of the additions since 1840, are technical words, the use of which is quite common among educated people.

The only conclusion I feel at liberty to draw is, that Marsh's numbers are quite too small, and that 30,000 words is not at all an unusual vocabulary.

The further pursuit of this subject has great interest, but I feel obliged to leave it, at this point, to the philologists, who are more peculiarly concerned. I shall hope that this slight paper may call out remarks from those members of the Society who are better informed upon this subject than I can be.

NOTE.—Since writing the above the Hon. George P. Marsh has written a letter to the New York Nation, in which he states that in giving estimates of the vocabularies of men of various classes he used *word* "in the sense in which, in such discussions, all philologists would agree in employing it," that is, "in estimating the number of *words* I took only the simple or stem and not the inflected forms of the vocables."

This of course explains the difference between Mr. Marsh's estimates and my own conclusions in the preceding paper, but I have been induced to allow that paper to remain in its present form, as it is an attempt to get a practical idea of the number of words, in the sense in which I use the term, which are in common use (counting, for example, *lover*, *loveless*, and *lovely*, as three words, although they have the same "simple or stem"). In this way we obtain a knowledge of the number of signs for ideas, and the research may be of interest although not of philological value.

I am the more inclined to leave the conclusions as they are, as Prof. Eastman, U S. Navy, starting from the same basis, has fully and carefully confirmed my principal conclusion, viz.: that many men have vocabularies of over 30,000 words, and he has shown that the probable error of his estimate is less than one per cent.

May 30, 1875.

VII.

ON THE MOVEMENTS CAUSED IN LARGE ICE-FIELDS BY EXPANSION AND CONTRACTION, AS ILLUSTRATIVE OF THE FORMATION OF ANTICLINAL AND SYNCLINAL AXES IN GEOLOGICAL FORMATIONS

By MONTGOMERY C. MEIGS.

(Prepared April, 1869. Read February 27, 1875.)

Hearing the discussion of the National Academy upon the section of the Appalachian formations contributed by Mr. J. P. Lesley, N. A., I was led to recur to certain phenomena which were daily presented to my observation during two severe winters spent at Rouse's Point on the shores of Lake Champlain in latitude 45° N.

The winters in that region are severe. The thermometer is frequently below 0 of Fahrenheit for days together. It seldom descends below —30°, but —18° is a not uncommon temperature. The lake is fed by streams which rise in the Adirondack and Green Mountains, deriving their supplies from the heavy snows of those ranges, where four feet of snow on a level is not uncommon. Its waters are clear, cold, and still. No current exists to move the ice, which is produced early and remains late. Its thickness, I judge, averages about twenty-four inches. The lake is irregular in form. Its shores and those of the islands it contains are generally rocky, with some beaches of drift gravel, sand, and boulders.

While at night the thermometer descends to —30°, during the day the sun's rays, shining through a dry, clear atmosphere, have considerable power. The ice, a two feet thick stratum floating freely upon the quiet water, lies between the water always at 32° and the air varying from +32° to —30° and further subject to the action of the direct rays of the sun. Its lower surface must always retain the uniform temperature of freezing water or melting ice (+32°). Its upper surface may take any temperature between +32° and approximately —30°. As ice, when once formed, is subject to the same laws of expansion and contraction as other solid bodies, the upper surface contracts under the low nocturnal temperature, producing a tension which is suddenly relieved by extended cracks. On a frosty night the great ice fields, 125 miles long and from 1 to 10 miles in width, are continually cracking with a rushing or roaring sound, which is one of the striking natural phenomena of this northern region. A crack sometimes starts apparently at the feet of a traveller on the ice, and its rushing

roaring sound will run off till lost in the extreme distance. This sound is almost continuous in very cold nights, and there must be millions of such cracks formed.

All these, sooner or later, by the hydrostatic pressure, are infiltrated with water, which, in the thin fissures, freezes immediately. They can be seen of all sizes from a mere crack to some inches in width where the ice has parted through its whole thickness and yielded to the contractile effort. When the temperature rises during the day, this cracking ceases. The ice expands to suit its increased mean temperature, and its edges encroach upon the shores more and more day by day. A permanent increase of size results from the filling of the contractile fissures by frozen water, and on all the beaches of the lake a ridge parallel to the shore is formed above the level of the water of the lake, composed of sand, gravel, stones, and even large boulders, which are each winter pushed further and further up the beach, until they reach the limit of the ice edge. This may be likened to a secular variation or expansion, its period being the existence of the ice field.

The daily expansion and nightly contraction, arising from the diurnal change of temperature of the ice, gives rise to effects even more striking and important to the residents on the shores than this annual variation. The lake is irregular in form. Its shores, and those of the islands with which it abounds, form wide bays or lobes of water, separated from each other by narrower straits, which are limited by opposite advancing or receding points or reefs of land or of rock. One bay or lobe three or four miles in width and several miles in length will be connected with the next by a narrower portion, perhaps only a mile in width. In the contraction and expansion of these great fields the weaker lines, or lines of least resistance, are across these constrictions, and it is along these lines, which are the same year after year, that the principal visible effects of diurnal expansion and contraction are to be observed. The ice breaks or parts on these lines, sometimes leaving an open crack or line of water several feet in width, difficult to cross on foot or in the carriole, as the northern sledge is called. The common winter road of the shore inhabitants is on the ice; and I have often, at certain well-known points, driving out early in the morning, found an open crack difficult to pass. Returning in the evening, after the heat of the day had produced its effect, the edges of the ice would be found to have met and inflected either upwards or downwards to such a degree that an axe would be needed to effect a passage.

This action is to be seen winter after winter at the same places, and the formation of the ridge or gutter is observed by all, for all are put to inconvenience and sometimes in peril by their movements. When the edges of the ice fields happen to bend downwards under the effects of expansion, the passage is most dangerous. I have seen horses, approaching too near the edge con-

cealed by a thin film of ice which is generally formed over the exposed water surface, slip into the water and lose their lives. Such accidents are common, though not often fatal. The horse is generally choked by a strap round his neck to stop his struggles, and while thus quieted he is dragged out by the efforts of the driver, and soon recovers on the slacking of the strap.

When the edges of the ice rise a passage is hewn by means of an axe. Looking at the Appalachian section of Mr. Lesley, it seemed to me that a section of these ice fields was a model, on a small scale it is true, yet several miles in length and width, of the rock strata which are there represented.

The same solid field floating upon a liquid ocean of constant or nearly constant temperature, exposed now on its upper surface to the atmosphere and to empty space, but at some former time covered by many thousand feet in thickness of ice, which has eroded the ridges and filled the valleys; in some places crimped by an evident increase in dimensions, in others forced up into ridges or depressed into valleys, anticlinal and synclinal axes, but these greater disturbances occupying only 175 miles of a cross section or profile 3000 miles in length.

The filling of the ice cracks by water which freezes is represented by the dykes of igneous or aqueous rock which abound in the geological profile. All these entered in a fluid condition, then solidified, and increased the dimensions of the field or formation.

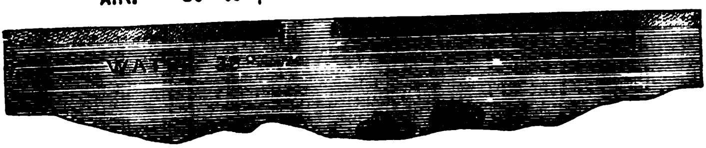
Changes of temperature, not diurnal as in the ice fields, but secular, would produce the ridges and valleys in the rocks as the diurnal changes produce their models in the ridges and gutters of the ice fields.

It is well to look for the smallest sufficient cause in reasoning upon observations of natural phenomena; and as Lyell holds that most of the changes on the earth's surface may be accounted for by causes and operations still seen in action, I have thought it worth while to record these observations, forced upon my attention during my daily rides for two seasons upon a frozen lake, as illustrating and accounting for a part of the contortions of the earth's surface.

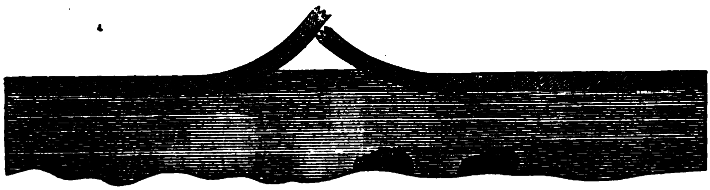
Some sketches on the accompanying page illustrate the more common physical phenomena herein described.

A. Crack in the ice on a cold night or morning.

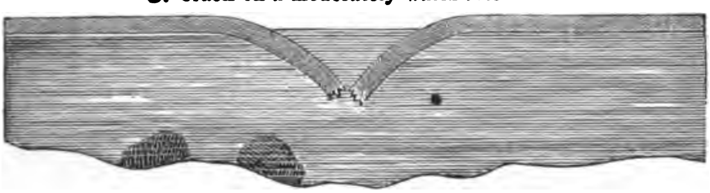
AIR. —30° to +32°



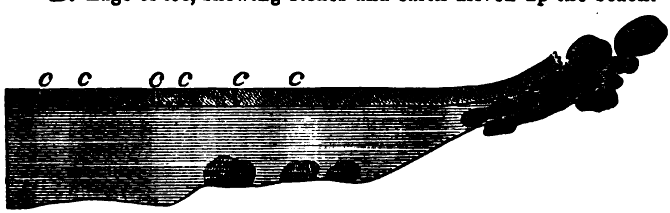
B. Crack on a moderately warm afternoon.



C. Crack on a moderately warm afternoon.



D. Edge of ice, showing stones and earth driven up the beach.



C C C C C C. **Cracks filled with water, and frozen.**

VIII.

DESCRIPTIONS OF NEW SPECIES OF FOSSIL PLANTS FROM ALLEGHANY CO., VIRGINIA; WITH SOME REMARKS ON THE ROCK SEEN ALONG THE CHESAPEAKE AND OHIO RAILROAD, NEAR THE WHITE SULPHUR SPRINGS OF GREENBRIER COUNTY, WEST VIRGINIA.

BY F. B. MEEK.

(READ JUNE 15, 1872.)

While on a visit last summer at the White Sulphur Springs of Greenbrier County, West Virginia, I saw, in the possession of a gentleman near that place, a beautiful specimen of a fossil fern, that had been found at Lewis's tunnel, on the Chesapeake and Ohio Railroad, some six miles southeast of the springs. Being much impressed with its elegant form, and fine state of preservation, I concluded to stop at the locality on my return home, with the view of examining the rocks, and collecting such specimens as could be found; and, while there, I succeeded in procuring the species described in this paper.*

The masterly preliminary reports and papers of Prof. William B. Rogers, on the territory now composing Virginia and West Virginia, have rendered the grand general features of the geology of those States so familiar to most scientific readers, that any extended remarks on that subject are unnecessary here.† For the information, however, of those who may never have visited this interesting mountain region, as well as to convey a more clear idea of the geological horizon of the fossils under consideration, it may be proper, before proceeding to describe these plants, to state a few of the details of the geology and topography of the country immediately surrounding the springs, as well as for a few miles west of the same, and eastward along the railroad to the locality of Lewis's tunnel, where these fossils were discovered.

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* I am under obligations to Gen. W. C. Wickham, Vice-President of C. and O. Railroad, for a letter to the conductors of passenger trains, instructing them to stop and allow me to get off at any points I might wish to examine along the road; also to H. D. Whitcomb, Esq., Chief Engineer of the road, and to Maj. Peyton Randolph, Chief Assistant Engineer, for accurate maps of portions of the country along the same, and for other information. I am also indebted to Mr. J. J. Gordon, one of the contractors of Lewis's tunnel, and to Mr. Terrence McGlone, for fine specimens of the fossil plants found at that place.

† It is much to be regretted that Prof. Rogers's final reports on the Geology of Virginia, which I understand were prepared in much detail, were never published.

In the first place it may be stated that these springs are situated in a narrow valley, two thousand feet above tide, near the eastern margin of Greenbrier County, West Virginia, and also within a few miles of the dividing line between Virginia and West Virginia. A little to the northwestward of the inclosed grounds at the springs, which are not situated quite in the lowest part of the valley, flows Howard's creek, a beautiful, perfectly clear mountain stream, that runs westward into Greenbrier River, a tributary of the Great Kanawha. Almost immediately on the southeast side of the grounds, and at a little greater distance across the valley to the northwestward, mountains, clothed with pines and various deciduous trees, rise from twelve to fifteen hundred feet above the valley; that to the northwestward being composed, at least near its base, of shales and flags of the age of the Hamilton Group (including the Marcellus shale) of the New York series; while that on the southeastward, for five or six hundred feet above its base, is composed of the same formation, with heavy beds of Chemung strata above, the whole dipping at a high angle to the southeast, and containing many characteristic fossils. To the southward Kate's Mountain is in sight, at a distance of two miles; while Greenbrier Mountain bounds the view on the west, within a mile or so of the springs. Four to five miles to the eastward, the Alleghany Mountains proper occur, the springs being west of the principal crest of this range, in the midst of a district abounding in mineral springs of various kinds and temperatures.

The grandeur of the scenery of this region, its pure mountain air, always comparatively cool and pleasant during the hottest part of the season at this altitude, together with the well-known medicinal properties of its waters, and the elegant and ample preparations for the accommodation of large numbers of visitors, render this a delightful place for invalids and seekers of pleasure and comfort to while away the sultry months of summer.

As stated by Prof. Rogers, these springs issue directly from a local uplift of rock of the age of the Oriskany sandstone of the New York series; but so near the junction of this with the overly lower black shales at the base of the Hamilton group, as to render it probable that the water derives its sulphurous properties, and possibly some of its salts, from the latter.*

* According to Prof. Rogers's analysis, the solid matter left by the evaporation of 100 cubic inches of this water, at a temperature of 212° Fah., was 65.54 grains, composed as follows:—

Sulphate of lime	31.680	grains.
Sulphate of magnesia	8.241	"
Sulphate of soda	4.050	"
Carbonate of lime	1.530	"
Carbonate of magnesia	0.506	"
Chloride of magnesium	0.071	"
Chloride of calcium	0.010	"

The exposed portions of the Oriskany beds here are not, as is often the case further north, composed of sandstones, but consist, at least mainly, of a rough, yellowish-gray mass of highly cherty strata, in some parts passing almost into a quartz rock. Although little exposed at this place, this rock evidently forms almost the entire bulk of a low hill, or ridge of oval form, and a few hundred yards in length, included as a part of the north side of the ornamented grounds about the springs. This hill is depressed on top and covered by a natural growth of shade trees, and has been tastefully laid out into walks and winding paths, provided with occasional rustic seats for the accommodation of visitors. Its summit is perhaps not more than ninety to one hundred feet above the lowest part of the valley on the north, around which side it is more or less precipitous; while to the southward it slopes down more gradually to the lower parts of the grounds, laid out into winding walks and drives, with intervening spaces of grassy sward, shaded at intervals by clumps of spreading oaks, elms, and other trees. Along the entire length of its southern slope a

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Chloride of sodium	0.226 grains.
Protosulphate of iron	0.069 "
Sulphate of alumina	0.012 "
Earthy phosphates	a trace
Azotized organic matter, blended with a larger proportion of sulphur, about	0.005 "
Iodine, combined with sodium or magnesium, a trace.	

The volume of each of the gases in a free state in 100 cubic inches of the water, he found to be as follows:—

Sulphuretted hydrogen	0.66
Nitrogen	1.88
Oxygen	0.19
Carbonic acid	3.67

The water is perfectly clear, and flows copiously; and, although appearing cool to the taste when drank during the warmer part of a summer's day, it is, as first shown by Prof. Rogers, properly speaking, a thermal water, its temperature, though somewhat variable, never being less than about nine, and sometimes as much as nearly thirteen degrees Fah., above the mean annual temperature of the air at the locality and altitude. That is, its temperature varies from 61° to 65° Fah.; while the mean temperature of the air, as determined by seven years' observations under the direction of the Smithsonian Institution, at Lewisburg, a few miles west of the springs and at a little lower elevation, is, I am informed by Prof. Henry, 52.2° Fah. Most of the mineral springs of this region, especially those that issue from anticlinal axes of the strata, are, as observed by Prof. Rogers, thermal waters, from which fact we may infer that they most probably arise from considerable depths, and owe their temperature to the internal heat of the earth.

The White Sulphur is, I believe, the only proper thermal water in the State, that is at the same time rather strongly impregnated with sulphur. When freely drank, it acts as a mild cathartic and diuretic; but its most valuable properties are its alterative powers in chronic diseases of various kinds, for the relief of which it has long been celebrated.

continuous excavation has been made to form a terrace, for the reception of a long row of neat one- and two-story cottages, which are, at places, almost hidden from view by shade trees.

At the eastern end of this terrace, just behind the cottages, as well as along the broad walk winding around that end of the hill, the dark Devonian shales belonging at the base of the Hamilton group of the New York series, are seen dipping off at a high angle to the southeastward. But on following the terrace westward behind the cottages, we soon come to a low nearly continuous outcrop of the Oriskany beds, dipping at the same high angle as the shale mentioned, to the southeastward. This rock can be traced to the west end of the hill on this side, and also forms high precipitous exposures around its northern side, one of which has been fancifully called the "lover's leap." At the western base of the hill, it is likewise again seen in the walk leading down to the bath-houses, where it presents almost a flinty appearance. Again it appears just below the principal spring, some ten or fifteen feet higher than at the bath-houses, forming the bed of the little stream running from the spring—being here, in places, whitened by the deposit of hydrated sulphur left by the water trickling over its surface. The bottom of this spring is also formed of this rock, and it is a little exposed along the side of a road, at a somewhat higher elevation, about forty or fifty yards south of the same.

This last seems to be about the end of the exposed part of this little uplift of the Oriskany formation here, in a southwestward direction, the overlying shales being met with in a hill on this side behind another row of cottages situated along its northeastern slope.

A low naked knob, only about twelve feet in height, of the lower black shales, is also seen on the immediate margin of Howard's creek, some sixty or seventy yards west of the springs, which, as already intimated, are situated at the western and lowest part of the grounds. This exposure is hardened, contorted, and crumpled as if it had been kneaded together by some powerful agency while in a yielding or semiplastic condition.

Another elevation at the northeast side of the grounds, called "Prospect hill," rises gradually to about the height of that already mentioned on the north, and is also covered by shade trees and laid out into walks; its southwestern slope being likewise occupied by a row of elegant two-story cottages; which, with those already mentioned, and others on the south side, surround the central part of the ornamented grounds in which the large hotel is situated. So far as I could see, this last-mentioned hill seems to be composed entirely of the shales and flags of the Hamilton group, of different shades of color.

The exposures here show that the strike of this little uplift of Oriskany and the overlying shales, is northeastward, and south-

westward, or parallel to the general trend of the ranges of the whole Appalachian region, as is most generally the case (local flexures excepted) with the anticlinal axes throughout this district.

As there are no corresponding Oriskany beds seen just on the northwest of this uplift, dipping in the opposite direction, and it is evident that such material could not have been worn away by Howard's Creek, the immediate valley of which directly intervenes between this hill and the mountain, composed mainly, if not entirely, of Hamilton and perhaps Chemung group beds on that side, it would seem that there may be a slight local inequality in the elevation of the strata here, along the opposite sides of a fracture. Whether this axis brings to view the Oriskany beds further northeastward, along the opposite side of the valley on the line of strike, I did not ascertain by personal observation, as I did not examine the mountains in that direction. I infer, however, from Prof. Rogers's remarks that it does, and this would indicate an oblique fracture of these beds, because the valley of Howard's Creek, which crosses the strike obliquely, could hardly have been cut through such a rock by that stream.

For some time I was unable to find any recognizable fossils in the Oriskany beds here, though I had seen some obscure casts and moulds of brachopods in the cherty beds along the little stream running from the springs. After diligent search, however, I succeeded in finding, near the bath-house, behind the cottages, at the west end of the hill above mentioned, imperfect casts of the well-known Oriskany shells *Spirfer arenosus*, *Meristella lata*, and moulds of *Rensselaeria ovoides?*

The deep cuts of the Chesapeake and Ohio Railroad through the spurs and ridges of the mountains along the south side of the valley here, afford a very fine opportunity to study the Hamilton group shales and more or less slaty beds, which seem to be of considerable thickness, and from near the springs dip at various angles to the southeastward, excepting where they are locally flexed and contorted. As the railroad runs close along the south side of the grounds, some of these deep cuts are within a few hundred yards of the hotel. One of these, in a direction nearly south from the springs, and almost on a line with the strike of the Oriskany uplift, but at a higher elevation than the nearest exposures of this rock immediately at the springs, shows the black Hamilton shale at the bottom, much contorted, with many polished surfaces caused by the slipping of one part upon another at the time of the upheaval, or during other disturbances of the beds. As freshly laid open by the excavations in progress when I was there, these dark shales emitted, under a noon-day sun, a sulphurous odor, suggesting the probable origin of the sulphuretted hydrogen of the springs, that have their source, as already stated, near the connection of these shales with Oriskany formation.

I saw no traces of any kind of organic remains in these lower dark shales, excepting a few trails, apparently of annelids, but they doubtless owe their dark color mainly to minutely comminuted particles of organic matter, perhaps chiefly of marine plants. From their position, however, and general appearance, there is little or no reason to doubt that they represent the Marcellus shale of the New York series; which, although sometimes viewed as a distinct formation, may perhaps be properly considered a subdivision of the Hamilton group. Here these dark beds are seen to shade upward into various lighter colored shales, and flags, presenting different shades of drab, olive, and dull gray, and bluish-gray. In some parts there are intercalated layers of various thickness and harder texture, composed of variable proportions of arenaceous and argillaceous matter. These latter harder layers are usually of dull gray color, or often on fresh fractures, bluish-gray, and, as may be seen in other cuts further eastward and westward, increase in proportion to the more shaly portions as we ascend in the series. At some places, however, higher in the series there are seen beds of dark shale. The lighter colored shaly beds above the lower dark shales are often quite soft, and are dug out along the railroad in small rhomboid blocks that soon crumble under exposure to atmospheric agencies.

Fossils seem also to be rather rare here in the lighter colored beds of the Hamilton group, near the bases of the mountains, in the immediate vicinity of the springs, but I succeeded in finding, in some of the harder layers at several places along the cuts of the railroad, and up the side of the mountain to the southeastward, casts and moulds of the well-known Hamilton species, *Spirifer mucronatus*, and *Orthis Vanuxemii*, along with *Martinia umbonata*, *Atrypa reticularis*, *A. aspera*, a flattened *Strophomena* and a smooth *Avicula* or *Pterinea*.

From what has already been said, it seems that there are here no representatives of the Upper Helderberg limestones or grits of the New York series; the black shales at the base of the Hamilton group being found resting directly against and upon the Oriskany.

West of the Springs, the lower dark shales are seen along the base of the mountains for a mile or more, on the right or northwest side of the valley, dipping at high angles to the northwestward, or at places locally tilted vertically, or variously flexed and distorted as if by lateral pressure, as well as from upheaval. The direction of the valley here is southwestward, but within a short distance its direction becomes nearly east and west, and five miles below, it curves around more nearly to the north. The railroad sweeps around the south side of this curve, cutting through several spurs and ridges of the mountains on that side of the valley, at an elevation of some fifty feet above its bottom. Its direction for several miles below the springs being very obliquely

across the axis of elevation, the cuts continue in the lighter colored shales and harder layers, which are at places seen contorted and dipping locally in different directions. As the road curves around to the northwestward, however, it crosses the strike of the strata less and less obliquely, so that, although a descending grade, it rapidly passes from (geologically) lower to higher strata, as it turns more nearly in the direction of the dip.

I found Hamilton types of fossils for a mile or more below the springs, but beyond this my examinations in that direction were not sufficient to determine exactly where the Hamilton ends, in going down the valley. To the westward, the harder less shaly beds were noticed to increase, but no very abrupt or strongly marked lithological changes were observed near the bases of the mountains, until about four to four and a half miles below the springs, by the curve of the road, near which point some whitish, rather coarse sandstone, at places containing pebbles of white quartz, was seen along the sides of the mountains, in rather massive beds, dipping at a high angle to the northwestward. Some half mile or less further on, in a nearly northwestward direction, the dip brings this sandstone down to the bottom of the valley. A deep cut at this place, at the entrance of a tunnel some forty to fifty feet above the sandstone, penetrates hard bluish-gray, more or less gritty beds, alternating with softer crumbling reddish, and, in places, greenish strata, in which argillaceous matter seems to predominate.* I saw no fossils here, excepting fragments of black vegetable matter, but I was impressed with the resemblance of these beds, and the red clays some of them form by disintegration, to some of those seen in the Catskill Mountains of New York, formerly referred to the Old Red Sandstone, but which, since Col. Jewett's discovery of Chemung fossils high in those mountains, have been mainly included in the Chemung group of the New York Devonian. I have the impression, however, that the beds penetrated by this excavation are at least as high in the series as the Old Red, or possibly somewhat higher, as there must be, owing to the dip here, a very considerable thickness of strata between them and the Hamilton group seen further up the valley. Being at the time in rather feeble health, I did not attempt to make the necessary examinations to ascertain the exact limits of the groups here, and only allude to the rocks seen in this cut, on account of their close similarity in lithological characters, to those containing the plants described in this paper from Lewis's tunnel, about six miles to the southeast of the springs; especially as the reverse of dip, to the southeastward from the springs to the last-mentioned locality, would also indi-

* It is probable that these beds and the whitish sandstone seen below them, owing to the general inclination of all the rocks here, rise to the summits of the mountains, some miles further eastward, on the west side of the valley, and nearer the springs, than where I saw them.

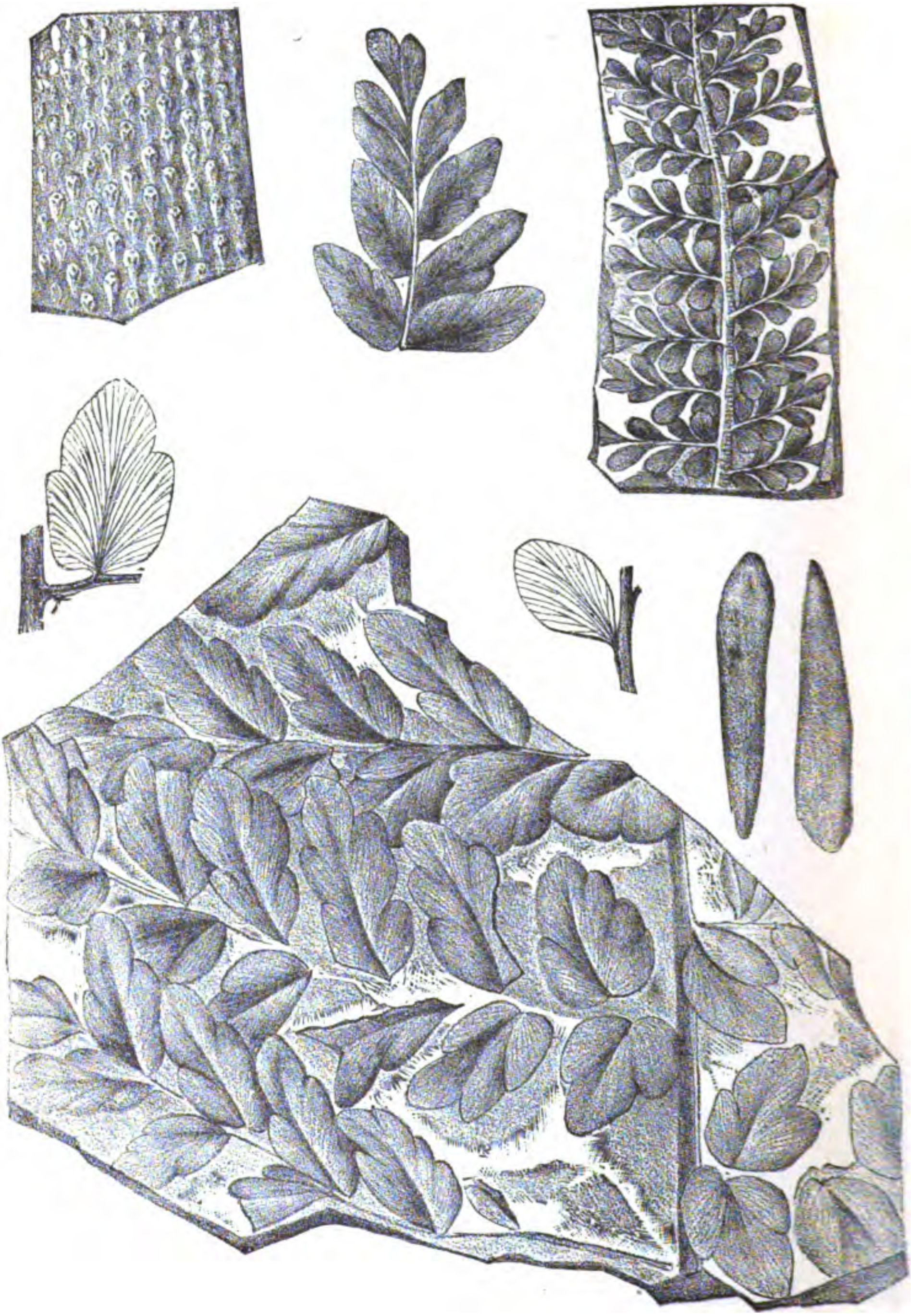


PLATE I.

Fig. 1. LEPIDODENDRON SCOBINIFORMIS.

A part of one of the smaller branches, showing the character of the surface scars of the same.

Fig. 2. CYCLOPTERIS (ARCHÆOPTERIS) ALLEGHANENSIS.

2*a*. A part of a frond, natural size.

2*b*. One of the pinnules magnified, to show the nervation.

Fig. 3. CYCLOPTERIS VIRGINIANA.

3*a*. Part of a frond, natural size.

3*b*. Outer extremity of one of the pinnæ.

3*c*. One of the pinnules magnified, to show the nervation.

Fig. 4. CARPOLITHES.

PLATE II.

Fig. 1. CYCLOPTERIS (ARCHÆOPTERIS) LESCURIANA.

1*a*. A part of a frond, upper side, reduced somewhat more than one-eighth diameter in size.

1*b*. One of the pinnules magnified, to show the nervation.

1*c*. A small part of the rachis magnified, to show its rugose character.

cate that the beds at these two excavations occupy about the same position in the series.

Returning to the springs, which are situated in the axis of elevation, we find that several deep cuts and tunnels along the railroad, just east of the same, present fine sections of the Hamilton group shaly beds, with more or less of harder and more compact gray layers intercalated. The more shaly softer beds here present the usual light-drab and grayish tints, but at one place I noticed some very dark shale. Generally fossils seem to be rare here also, along the cuts of the road, though in some of the harder more arenaceous beds, within about one mile of the springs, I found casts of a few Hamilton types.

Between this and Alleghany tunnel, three and a half miles southeast of the springs by a right line, I only saw the rocks in passing along on the cars. As already stated, the dip of the strata east of the springs is to the southeastward, with the exception of local distortions, apparently all the way to Lewis's tunnel and beyond; and as the direction of the road between these two places is nearly, though not exactly, the same as the dip, in coming eastward, we ascend again rather rapidly in the series, the inclination of the strata being at a pretty high angle. With local exceptions, the beds become less shaly, with a larger proportion of hard layers in coming eastward. At the west end of Alleghany tunnel, which is seven-eighths of a mile in length, and at nearly the same actual elevation as the springs, I saw gray and olive shales, with some more compact arenaceous layers, tilted and much confused, some parts standing nearly in a vertical posture, as if crowded together by lateral pressure. Similar shaly beds seem also to occur at some points in the tunnel, as it has been found necessary to wall up and arch it over with masonry at places.

At the east end of this tunnel there is a long open cut, with vertical walls on each side, in which the strata are seen to be more compact, and show little of the shaly structure. They generally present a bluish-gray tint on fresh fractured surfaces, and dip to the southwestward at an angle of from 45° to 50° below the horizon. Where long exposed to atmospheric agencies, however, above the cut, on the slope of the mountains, they weather to a light yellowish-gray color, but sometimes show rusty surfaces, when broken. At one place, a little above the east end, and on the south side of this cut, I found a mould of the ventral valve of a *Spirifer* agreeing exactly with that of the more extended forms of *S. mucronatus*. Associated with this, however, were numerous casts of the interior, and moulds of the exterior, of the Chemung species *S. mesacostalis*, agreeing in all respects with the transversely extended variety of that shell, as found in New York, not only in form, surface markings, and the characteristic mesial rib, but also in having a deep slit in casts of the

rostral cavity, left by a prominent, narrow, internal ridge or septum, similar to that seen in *Spiriferina*. In the same beds with these, I also found several casts agreeing well with the Chemung species *Leiorhynchus mesacostalis*, and *Orthis impressa*, together with the Chemung and Hamilton forms *Atrypa reticularis* and *Martinia umbonata:* likewise a small hemispherical *Productus*, a small plicated *Rhynchonella*, and an *Avicula* or *Pterinea*, like *A. spinigera*.

From such an assemblage of fossils, it can scarcely be doubted that these beds belong to the horizon of the Chemung group of the New York Devonian series. It is true, *Spirifer mucronatus*, is, I believe, not there known above the horizon of the Hamilton group, but Prof. Henry D. Rogers states that it occurs in the Chemung in Pennsylvania, while the associated species form together a group of fossils nowhere, so far as I am informed, ever found below the horizon of the Chemung.

About half a mile east of the locality where the above-mentioned fossils were found, I collected from loose pieces of fine-grained, gray, somewhat gritty rock, along the bed of a little mountain stream, a *Schizodus* apparently identical with a New York Chemung species; and from a cut a few hundred yards further eastward, from a similar rock in place, several bivalves like Chemung forms, along with casts of the well-known Chemung species *Spirifer disjunctus*.

The beds all along here continue to dip at the same high angle to the southeastward, and become rather more gritty in that direction; while immediately east of the last-mentioned locality, small masses of whitish, more or less pebbly sandstone had slidden from the slope above the road. This material seems, however, scarcely to form a continuous bed here, but apparently passes into fine-grained, hard gray rock, nearly or entirely without pebbles. This, more or less pebbly and at places whitish grit, is very probably the same seen dipping to the northwestward, five miles below the springs on Howard's Creek, though here it seems to be much less developed as a distinct mass from the other beds.

At a point some three-fourths of a mile east of the Alleghany tunnel, exposures were seen along the road, of rather massive beds of hard, bluish-gray, more or less gritty rock, alternating with softer crumbling material of brownish color, the whole being much like the beds seen in the cut five miles below the springs. At one place, thin local seams of dark shale, and some little coal were seen intercalated among these rocks. A little east of this the road curves around to the left, in a northeast direction, and enters a long open cut, leading to the southwestern end of Lewis's tunnel; and it was at the bottom of this excavation that the plants under consideration were found.

The base of this excavation is here perhaps some twenty odd feet lower than the exposures containing the thin seams of dark

shale and coaly matter already mentioned; but I am inclined to think it very nearly, if not exactly at the same geological horizon, as the dip of the strata would apparently bring those seams down to this level. The direction of the cut being more nearly parallel with the strike of the strata than the general course of the road west of this curve, the beds dip more obliquely across the excavation, and at the point where the laborers were at work when I was there, a thin seam of black, more or less shaly matter, containing at places a few inches of coal, was seen passing across the bottom of the cut. This seam of bituminous shaly matter is very irregular in thickness, being in some places a foot or more thick, but soon thinning out to a few inches. The included coal is also even more irregular, being sometimes several inches in thickness, and again thinning to a mere streak of black bituminous shaly matter, or sometimes entirely disappearing. Where pure and not crushed,* it often presents a somewhat lustrous appearance like anthracite, but it burns with a bright flame that shows it to be bituminous or semi-bituminous. Of course it does not exist in sufficient quantities to be made available for any practical purposes, but its occurrence here among these older strata, so far beneath the horizon of the true Coal measures, and in connection with so many beautiful fossil plants, is, to the geologist, an interesting fact, as it shows that similar physical conditions to those that gave origin to our great widely extended coal-beds of the later Carboniferous period, prevailed locally here, at least for a comparatively brief period of time, long before the true coal-producing epoch.

The plants found associated with this coal occur both in the more or less dark-colored shaly matter, and in the fine-grained argillaceous and slightly gritty harder rock just below it, as well as above. The wonderfully perfect condition of the most delicate fronds of the ferns found here, shows that these plants could not have been drifted any great distance, by streams or ocean currents, before being buried beneath the fine sediment now forming the rocks in which they are imbedded, but that they must have grown at least near the locality where they are now found. Hence it is evident that while the vast accumulations of sedimentary matter composing these mountain masses, were being deposited upon a gradually sinking ocean bottom, there were shores, and perhaps islands near, that supported a growth of terrestrial vegetation. Indeed it is probable that even at some of the very spots where the coal is found, the bed upon which it rests was raised slightly above the surface of the sea, and that most of the plants of which the coal is formed, as well as those with which it is associated, may have grown very nearly, or pos-

* Being a much softer material than the hard rocks above and below, this seam of coal and shaly matter has, at some places, been crushed by the movements of the beds under tremendous pressure.

sibly in some cases exactly where they are now found. Of course there were many subsequent oscillations of level, by which much of our continent was sunk deep enough beneath the ocean level, to receive thousands of feet in thickness, of later deposits, and again raised to its present elevation.

The walls of the excavation at the bottom of which these plants were found, are composed of the same fine-textured, more or less hard, gray and bluish-gray, argillaceous, slightly gritty beds, as some of those containing the plants, for ten or twelve feet above the bottom of the cut; and farther up, apparently much the same kind of rocks continue for thirty or forty feet, alternating with beds of softer, crumbling, brownish-red material, disposed to form red clays by disintegration. The nature of the rocks composing the mountains here, above this last-mentioned horizon, was not determined by examination; and no organic remains, excepting those of the plants collected here, were seen at this locality.

About two hundred yards to the northeastward from the point where the plants already mentioned were taken out at the bottom of the cut, excavations were in progress in the tunnel, by means of a vertical shaft sunk on an elevation more than one hundred feet above the actual horizon of the point where the plants alluded to above were found. The rock thrown out of this shaft is a very hard, compact, rather coarse-grained, massive grit, of a light bluish-gray color, differing from any of the beds exposed in, or directly over, the cut at the plant locality. It is brought up from the shaft generally in large, irregular massive blocks, as blasted from the beds. In these I saw many fragments of stems and branches of trees, most of which are small, but I obtained several specimens of moderate size, one of which consists of a fragment broken at both ends, measuring twenty-two inches in length, and three to four inches in diameter. Some crushed examples seen in the rock appear to have belonged to individuals of considerably larger size. All of these specimens are coated, as it were, by a bark-like covering of shining coaly matter, while inside of this nothing but the same hard, gritty material composing the surrounding matrix occurs. Generally no well-defined markings are seen either on the surface of this coaly matter, or on the rock within. On one of the specimens, however, obtained here, there are pits closely resembling, in size, form, and arrangements, those of the genus *Stigmaria*.

The absence of surface markings on most of these specimens is perhaps due, in part, to the fact that they were drifted and consequently abraded before being deposited here, and in part to the tremendous pressure to which they have been subjected during the consolidation of the rock, or its subsequent movements. The evidences of pressure are seen on nearly all the specimens, which are usually found crushed and broken, with the surface of the

shining coaly covering polished and striated by the slipping of contiguous portions of the matrix under great pressure.

Owing to the fact that nearly or quite all of the plants I obtained here in a condition to show their specific characters, seem to be new species, while no other organic remains of any kind were observed in these beds during my rather limited examination, we scarcely have the means of determining their exact horizon in the series. The affinities of the several species of ferns found in the bed at the bottom of the cut, at this place, would, however, favor the conclusion that they belong near the junction of the Old Red Sandstone and the lower Carboniferous, but probably in the latter.

That the remains of Chemung types of shells occur at lower stratigraphical positions at several places between here and Alleghany tunnel, has already been stated. There must, however, be a considerable thickness of strata intervening between these two points, the dip being all along here, I should think, scarcely less than 30° to 40° below the horizon, and perhaps at some points more, to the southeastward. I made no measurements of distances, angles of dip, or of the thickness of strata (having no instruments), but the distance between the two tunnels, by the curve of the road, is, I was informed, about one and a half miles. A straight line between these two points, however, would not be in the direction of the dip, but obliquely across the strike, and something less.

The distance, by a right line, between the locality where I found the last Chemung fossils, coming eastward, and the point where the remains of the plants were found in the cut at Lewis' tunnel, I should think little more than half a mile; and, making allowance for the direction of this line with relation to the dip, there would seem to be scarcely less than 1500 feet of strata, and possibly more, between the horizons of these two points. How much if any of this space may be occupied by Chemung rocks remains to be determined. That the Chemung extends from the furthest eastward point at which I found its characteristic fossils, back to Alleghany tunnel, however, where the same types occur, there can be no doubt, and there appears to be good reason to believe that there are from 1200 to 1500 feet of these rocks between these two points. Whether or not the Chemung extends back into Alleghany tunnel, I did not ascertain. I think it probable, however, that at least a part of the strata penetrated by this tunnel belongs to the horizon of the Portage group, because among the material brought out of its eastern end, I saw many thin slabs, of bluish and greenish tinge, showing, on their slightly glazed surfaces, fucoidal markings very similar to *Fucoides graphica*, so characteristic of the Portage group in New York. There is ample space between this point and the White Sulphur

Springs, for great developments of the Portage and Hamilton groups, if both exist here.

The thickness of the Chemung group was formerly estimated at about 1500 feet in New York; but from Col. Jewett's discovery, that a considerable thickness of the strata forming the Catskill Mountains, that had for a long time been referred to the Old Red Sandstone, really belongs to the Chemung, we may perhaps infer that 1500 feet is considerably below the maximum thickness of the latter formation in New York. Prof. Henry D. Rogers estimated its greatest thickness in Pennsylvania at more than 3000 feet.

From all the facts observed, I had at one time supposed that the plant bed at Lewis's tunnel holds a position in the upper part of the Devonian; but as Prof. Rogers informs me that the Old Red, if it exists there, is probably but little developed, the position of these plants may be more properly within the inferior part of the lower or subcarboniferous series.

Fossil Botany not coming within the range of my own especial department of investigation, my object in studying these plants was, at first, merely to identify the species, which it was supposed had probably been described. After making extensive comparisons, however, with the figures and descriptions in a large number of publications, without finding any species agreeing with them, I arrived at the conclusion that they are new, and decided to name and describe them. The specimens, however, have been submitted to Prof. Lesquereux, and afterwards to Dr. Newberry, as well as in part (with tracings of others), to Prof. J. W. Dawson, of Montreal, all of whom are well known to be high authorities on fossil botany; and these gentlemen concurred in the opinion that the species are new; though they differed somewhat in opinion respecting the generic affinities of the ferns, which happen to be types standing, as it were, intermediate between several of the established genera. This peculiarity of these forms, and the fact that the most important generic character (the nature of the fructification) can very rarely be seen in specimens of these older types of fossil ferns, render their classification difficult, and give origin to conflicting opinions, among the most careful and conscientious observers, respecting the generic names under which the species should be ranged.

I take pleasure in acknowledging my obligations to Prof. Dawson, Prof. Lesquereux, and Dr. Newberry, for the suggestions alluded to above, respecting these plants.

Lepidodendron scobiniforme, M.

Pl. I, fig. 1.

Cicatrices of smaller branches moderately distinct, small, or about 0.14 inch in length, and 0.09 inch in breadth, subovate in form, or rounded above and tapering to a mucronate point below,

placed in the usual obliquely ascending rows so as to present a quincuncial arrangement, smooth below. Interspaces smooth, somewhat less than the breadth of the cicatrices, measuring transversely, and half their breadth measuring in the direction of the oblique rows. Leaf scars small, placed at the upper end (and usually a little excentric to the right) of the cicatrices, subrhombic, about as wide as long, with upper side convex in outline, the lateral angles rounded, and the base abruptly pointed; sometimes with the entire outline subcircular, smooth, or without any visible vascular pits within.

The above description is taken from a portion of a flattened branch about an inch and a half wide, showing the cicatrices quite distinctly. But these markings present a great diversity of appearances on different portions of the different sized branches and trunks; and, consequently, the description would not apply to all of its parts. In some of the impressions of still smaller branches, or individuals, the cicatrices are more crowded laterally, more elongated, proportionally narrower, and, as seen in a cross light, present a decided elongate-rhombic outline, the interspaces being proportionally narrow, so as to make the cicatrices appear as if acutely pointed, both above and below. In this aspect, the leaf scars are scarcely seen, and the whole surface presents much the appearance of the figure of *Sigillaria Chemungensis*, given on page 275 of the Report on the fourth Geological District of New York. Even in these specimens, however, when viewed in a different light, the cicatrices can be seen to be really more or less rounded above, and the leaf scars obscurely defined. On still larger branches, the cicatrices become more and more faintly defined, and the leaf scars proportionally more distinct and more scattering, so that the surface looks very much like that of a *Stigmaria*. In following the markings to larger and larger branches, or individuals, the cicatrices are seen gradually to become obsolete, and longitudinal ridges begin to be developed. On fragments, apparently of the trunk of the same tree, these ridges are found to be from 0.25 to 0.46 inch in breadth, nearly flat (with sometimes very obscure traces of irregular longitudinal striæ), and separated by narrow irregularly interrupted furrows; while a single row of the small scars occurs along the middle of each, separated by intervals of about 0.50 inch. Again other specimens, apparently of portions of the trunk, show the ridges to have become obsolete or nearly so; but the leaf scars are still seen, more widely separated, and more obscurely defined. These longitudinally ridged specimens, therefore, present very nearly the characters of *Sigillaria*. Hence, it becomes a matter of some doubt to which one of the three genera, *Lepidodendron*, *Stigmaria*, or *Sigillaria*, the species should be referred.

It is true that the specimens seen are not in such a condition as positively to demonstrate that they all belong to the same

species—that is, no one individual tree has been seen entire, and showing all of the characters mentioned—but the specimens were found flattened together in the same matrix, and present such an uninterrupted series of gradations as to render it impossible to separate them; and to leave the impression on the mind that they really belong to the different parts of the same species.

Prof. Lesquereux has also informed me, that after figuring and describing his *Stigmaria minuta*, of the Pennsylvania Report, from the Lower Carboniferous of the State, he found other specimens clearly showing very similar gradations in the surface markings, and yet under circumstances rendering it positively certain that they all belong to one tree.

So far as I have been able to see, the markings on decorticated surfaces all become nearly obsolete.

In the same matrix numerous very slender grass-like leaves occur that probably belong to this species. The widest of these are not more than 0.13 inch in breadth, while some of them can be traced to a length of more than seven inches, and yet they are broken at both ends, and appear to be simple and almost of the same breadth throughout the entire length. They are always flattened by pressure, and generally show no very well-defined median vein, but in some cases they appear to exhibit traces of about four longitudinal lines, or veins.

STIGMARIA ? (sp. undetermined).

The specimens of this fossil in the collection are more or less compressed laterally by accidental pressure, and surrounded by a thin bark-like covering of shining coal. Generally they show scarcely any traces of surface scars; but one of them about 19 inches in length, with both ends broken away, and measuring at the larger end (which rather suddenly enlarges), 3, by a little more than 5½ inches in diameter, and at the smaller 2.40 by 4.30 inches in diameter, retains the scars or pits on the decorticated surfaces, with some degree of distinctness. These are alternately arranged in obliquely ascending rows, and are simple, vertically elongated depressions, deepest in the middle, and becoming rapidly shallower and narrowed to nothing above and below. In the direction of the spiral rows, as well as transversely, they measure about 0.40 inch from the middle of one to that of the next; while the interspaces are sometimes obscurely and irregularly a little wrinkled longitudinally.

The whole interior, within the surrounding bark-like coating of coal, is merely composed of the hard, rather fine gritty material composing the surrounding matrix, and shows no traces of an eccentric pith. This latter fact and the rather elongated form of the surface pits, without any ring or elevated point within, render it doubtful whether or not this form can be properly referred to the genus *Stigmaria*.

The specimens of this species do not occur directly associated with the other plants described in this paper, but at a somewhat higher geological horizon, about one or two hundred yards further eastward.

CARPOLITHES?

Pl. I, fig. 4.

These bodies may or may not be fruits, as they are too imperfectly preserved and defined to be satisfactorily determined. They seem to have been vesicular, or, at any rate, to have possessed little solid substance, as they are almost entirely flattened by pressure. As thus seen flattened in the matrix, they most generally present a spatulate outline, and vary in length from 1 inch to 1.60 inches, and from 0.20 to 0.30 inch in breadth, the widest part being generally near one end; while the opposite end is sometimes abruptly pointed, and the other usually more obtuse, or more or less rounded. They show no surface markings of any kind.

CYCLOPTERIS? (ARCHÆOPTERIS) LESCURIANA, M.

Pl. II, fig. 1, *a*, *b*, *c*.

Frond tripinnate,* attaining a large size, primary pinnæ lance-ovate or lanceolate in general outline, with a moderately stout, straight, somewhat rugose rachis. Secondary pinnæ regularly alternating, rather approximate, lanceolate, nearly straight or a little arched upward, with a slender, very slightly flexuous rachis, that diverges from the secondary one at distinctly less than a right angle. Tertiary divisions or pinnules regularly alternating, narrowed below to the short oblique petiole, the lower or inner ones being deeply divided into from three to five (rarely six) alternating, moderately divergent, narrow sublanceolate, simple, or rarely dentate leaflets; upper ones gradually becoming less and less divided, until they pass into merely slightly dentate, or simple lanceolate forms that are more oblique to, and slightly decurrent upon, the rachis. Nervation rather obscure; nerves not very numerous, moderately diverging, and apparently several times bifurcating.†

* The descriptions of this and the following species, are drawn up under the supposition that the largest specimens found are not fronds, but mere divisions of the same. If they should be found to be entire fronds, however, of course the description would have to be modified to correspond, as in that case the species should be described as bi-pinnate, and the division termed secondary pinna, would be primary, etc.

† In some of the specimens the upper side of the pinnules can be seen under a strong magnifier in a cross light, to be covered by numerous extremely minute, crowded longitudinal striæ, apparently independent of the nervation. These striæ can be traced down the narrowed base, or petiole, upon and along the rachis.

The specimens apparently belonging to this species before me, present considerable variations of form and other characters, some being decidedly narrower, with their pinnæ shorter, more distant, and more oblique, and their pinnules less divided. These, however, probably belong to different parts of the frond from that described here as the typical form of the species. Others have the pinnæ and pinnules, as well as the subdivisions of the latter, smaller and proportionally more slender, and presenting a more delicate appearance throughout. These latter may possibly belong to a distinct species, but they agree so nearly in all other respects with the form described as to leave the impression that the whole series belongs to one somewhat variable species.

This species has much the aspect of a *Sphenopteris*, to which Dr. Newberry thought it might be referred without impropriety. In this opinion Prof. Dawson was inclined to concur on examining a photograph of it. On a critical examination of its nervation, as seen in some specimens sent to him, he writes that he thinks it belongs more properly to the same group as *Archæopteris Halliana* (= *Sphenopteris laxa*, Hall), to which I had from the first supposed it to be related. Prof. Lesquereux, to whom I showed the specimens, also supposed the species to belong to *Palæopteris* of Schimper, which is the same as *Archæopteris*, Dawson, the name *Palæopteris* being preoccupied. Some other high authorities on fossil botany, however, have arranged similar forms under the names *Asplenites* and *Adiantites*.

From these remarks the student will readily understand that in the present unsettled state of opinion in regard to the limits between several of these older groups of fossil ferns, and the consequent confusion existing in their nomenclature, it is impossible to determine beyond doubt under what genus this species may ultimately have to be ranged, when all of these questions can be settled. It may therefore have to take the name *Sphenopteris Lescuriana*, or *Adiantites* (*Asplenites*) *Lescurianus*. Or, possibly, in case the name *Palæopteris* of Genitz should be found not to have been based upon a tenable genus, so that Schimper's name *Palæopteris* would have to replace *Archæopteris*, our species may have to be called *Palæopteris Lescuriana*.

Specifically this form will be readily distinguished from *Cycl.* (*Archæopteris*) *Halliana*, by wanting the row of broad separate pinnules along its rachis between the pinnæ as seen in that species, as well as by its more divided inner pinnules and more rigid pinnæ. Prof. Dawson thinks it more nearly related to his *C.* (*Palæopteris*) *Rogersi*, though, on comparison, he says he finds that the *Rogersi* has larger pinnæ, and more obtuse as well as larger pinnules, and a somewhat different venation.

Cyclopteris Virginiana, M.

Pl. I, fig. 3, *a*, *b*, *c*.

Frond apparently attaining a large size, and probably tripinnate. Primary pinnæ with a rather stout, rigid, smooth, or slightly striated rachis. Secondary pinnæ long lanceolate, regularly alternating, nearly straight, rather closely arranged, and standing nearly or quite at right angles to the rachis. Pinnules more oblique, rather approximate and regularly alternating; lower or inner ones shorter and broader than the others, abruptly narrowed, or apparently sometimes subcordate at the base, and attached to the rachis by an extremely short petiole, more or less distinctly trilobate, the lobes being obtuse, and broad-ovate in form; succeeding pinnules gradually becoming five-lobed, more elongated, or obtusely sublanceolate, more oblique, and less abruptly tapering at the base; beyond these, the others are less and less strongly lobed, or merely undulated on the margins, while a few near the extremities of the pinnæ are quite simple, still more oblique, and very gradually tapering to, and more or less decurrent upon, the rachis. Nervation distinct, nerves slender, palmately spreading, and bifurcating several times.

If specimens of this species, like the one figured, are imperfect primary pinnæ, and not fronds, it must have been a very large beautiful fern. It seems to have been much more rare than the last, as only the two specimens figured occur in a collection, containing fifteen or sixteen more or less imperfect examples of the last.

Although very distinct specifically from the foregoing, this seems, like that form, to stand as it were intermediate between several of the established genera. In some respects it is related to both *Sphenopteris* and *Cyclopteris*, while Prof. Schimper has included some similar forms in his genus *Triphyllcpteris*. Still other high authorities have placed apparently congeneric forms under the names *Adiantites* and *Asplenites*. It is therefore possible that when the affinities of the ancient types of ferns can be better understood, and the confusion that now exists in their nomenclature is corrected, the name of this species may have to be changed to *Sphenopteris* or *Triphyllopteris Virginiana*. I am not sure, however, that it should not be called *Archæopteris* (*Palæopteris*) *Virginiana*.

Cyclopteris (Archæopteris) Alleghanensis, M.

Pl. I, fig. 2, *a*, *b*.

Frond tri- or bipinnate. Primary pinnæ (or possibly the frond) narrow, or apparently lanceolate, with a comparatively strong, transversely wrinkled, rigid rachis, that is provided

with short, sublanceolate, regularly alternating, rather crowded pinnæ, directed nearly at right angles to its sides. Pinnules simple, alternate, very obtuse, and varying from subcircular to obovate, those nearest the rachis being sometimes nearly circular, and connected with the rachis by an extremely short petiole, or almost sessile; those further out narrower, more oblique, and tapering to a narrow base that is more or less decurrent on the rachis; terminal one sometimes a little larger than the smaller of the others, and partly confluent with the nearest of the latter. Nervation moderately distinct; nerves spreading from the base, and bifurcating two or three times.

This is probably a smaller species than either of the other two already described, and is very distinct from them both in the form and simplicity of its pinnules. But the single imperfect specimen of it figured was found, and it occurred directly associated with the others. In the form of its pinnules and their nervation it resembles *Archæopteris* (*Nœggerathia*) *minor*, of Lesquereux, but its pinnæ and pinnules are much more crowded and shorter.

For the reasons already explained, future corrections of nomenclature may require the name of this species ts be written *Adiantites* (*Asplenites*) *Alleghanensis*, or *Palæopteris Alleghanensis*.

IX.

ON SOUND IN RELATION TO FOG-SIGNALS, FROM INVESTIGATIONS UNDER THE DIRECTION OF THE U. S. LIGHT HOUSE BOARD.

By JOSEPH HENRY.

(Read December 11, 1872.)

(Before reading this paper Prof. Henry, as President, made the following preliminary remarks.)

The Committee of Arrangements have this evening called an extra meeting of the Society to embrace the opportunity to invite a few friends to meet Professor Tyndall. They have extended this courtesy to him as a mark of the high appreciation which the Society entertains of his scientific labors. As the worthy successor of Faraday, we recognize him as among the first contributors to the physical science of the day. Although this is an extra meeting, the proceedings will not differ essentially from those of ordinary meetings, but will consist in the presentation of communications purporting to be additions to knowledge, and in discussions regarding them. We trust that Doctor Tyndall, and the other invited guests, will join in the discussion, and in the communication of any facts, which the occasion may recall to memory, pertaining to the subjects under consideration.

The communication which I propose to make this evening is brought forward at this time especially on account of the presence of Doctor Tyndall, he being connected with the Light House system of Great Britain, while the facts I have to state are connected with the Light House service of the United States, and must therefore be of interest to our distinguished visitor. The facts I have to present form part of a general report to be published by the U. S. Light House Board.

The Light House Board of the United States has from its first establishment aimed not only to furnish our sea-coast with all the aids to navigation that have been suggested by the experience of other countries and to adopt the latest improvements, but also to enrich the Light House service with the results of new investigations and new devices for the improvement of its efficiency, or, in other words, to add its share to the advance of a system which pertains to the wants of the highest civilization.

Among the obstructions to navigation none are more serious, especially on the American coast, than those caused by fogs.

Fog, as it is well known, is due to the mingling of warmer

air surcharged with moisture with colder air, and nowhere on the surface of the earth do more favorable conditions exist for producing fogs than on both our Atlantic and Pacific coasts. On the Atlantic the cold stream of water from the polar regions in its passage southward, on account of the rotation of the earth, passes close along our eastern coast from one extremity to the other, and parallel to this but opposite in direction, for a considerable distance is the great current of warm water known as the Gulf stream. Above the latter the air is constantly surcharged with moisture, and consequently whenever light winds blow from the latter across the former, the vapor is condensed into fog, and since in summer along our eastern coast the southerly wind prevails, we have during July, August, and September, especially on the coast of Maine, an almost continuous prevalence of fogs so dense that distant vision is entirely obstructed.

On the western coast the great current of the Pacific, after having been cooled in the northern regions, in its passage southward gives rise to cold and warm water in juxtaposition, or, in other words, a current of the former through the latter, and hence whenever a wind blows across the current of cold water, a fog is produced.

From the foregoing statement it is evident that among the aids to navigation fog-signals are almost as important as light houses. The application, however, of the science of acoustics to the former is far less advanced than is that of optics to the latter. Indeed, attempts have been made to apply lights of superior penetrating power, as the electric and calcium lights, to supersede the imperfect fog-signals in use. When, however, we consider the fact that the absorptive power of a stratum of cloud, which is but a lighter fog, of not more than two or three miles in thickness, is sufficient to obscure the image of the sun, the intensity of the light of which is greater than that of any artificial light, it must be evident that optical means are insufficient for obviating the difficulty in question.

The great extent of the portions of the coast of the United States, which is subject to fogs, renders the investigation of the subject of fog-signals one of the most important duties of the Light House Board.

In studying this subject it becomes a question of importance to ascertain whether waves of sound, like those of light, are absorbed or stifled by fog; on this point, however, observers disagree. At first sight, from the very striking analogy which exists in many respects between light and sound, the opinion has largely prevailed that sound is impeded by fog. But those who have not been influenced by this analogy have in some instances adopted the opposite opinion—that sound is better heard during a fog than in clear weather. To settle this question definitely the Light House Board have directed that at two light houses on the route

from Boston to St. John, the fog-signals shall be sounded every day on which the steamboats from these ports pass the station, both in clear and foggy weather, the pilots on board these vessels having, for a small gratuity, engaged to note the actual distance of the boat when the sound is first heard on approaching the signal, and is last heard on receding from it. The boats above mentioned estimate their distance with considerable precision by the number of revolutions of the paddle-wheel as recorded by the indicator of the engine, and it is hoped by this means to definitely decide the point in question. We think it highly probable that fog does somewhat diminish the penetrating power of sound, or, in other words, produce an effect analogous to the propagation of light. But when we consider the extreme minuteness of the particles of water constituting the fog as compared with the magnitude of the waves of sound, the analogy does not hold except in so small a degree as to be of no practical importance, or, in other words, the existence of fog is a true but, we think, an insufficient cause of diminution of sound, which view is borne out by the great distance at which our signals are heard during a dense fog.

Another cause, which without doubt is a true one, of the diminution of the penetrating power of sound is the varying density of the atmosphere, from heat and moisture, in long distances. The effect of this, however, would apparently be to slightly distort the wave of sound rather than to obliterate it. However this may be, we think, from all the observations we have made, the effect is small in comparison with another cause, viz., that of the influence of WIND. During a residence of several weeks at the sea-shore, the variation in intensity of the sound of the breakers at a distance of about a mile in no case appeared to be coincident with the variations of an aneroid barometer or a thermometer, but in every instance it was affected by the direction of the wind. The variation in the distinctness of the sound of a distant instrument as depending on the direction of the WIND is so marked that we are warranted in considering it the principal cause of the inefficiency in certain cases of the most powerful fog-signals. The effect of the wind is usually attributed, without due consideration, to the motion of the body of air between the hearer and the sounding instrument: in the case of its coming towards him it is supposed that the velocity of the sound is reinforced by the motion of the air, and when in the opposite direction that it is retarded in an equal degree. A little reflection, however, will show that this cannot be the cause of the phenomenon in question, since the velocity of sound is so vastly greater than that of any ordinary wind, that the latter can only impede the progress of the former by a very small percentage of the whole. Professor Stokes, of Cambridge University, England, has offered a very ingenious hypothetical explanation of wind on sound, which we think has an important practical bear-

ing, especially in directing the line of research and subsequent application of principles.

His explanation rests upon the fact that during the passage of a wind between the observer and the sounding instrument the velocity of this will be more retarded at the surface of the earth on account of friction and other obstacles, and that the velocity of the stratum immediately above will be retarded by that below, and so on, the obstruction being lessened as we ascend through the strata. From this it follows that the sound wave will be deformed and the direction of its normal changed. Suppose, for example, that the wind is blowing directly from the observer. In this case the retardation of the sound wave will be greater above than below, and the upper part of the wave-front will be thrown backwards so that the axis of the phonic ray will be deflected upwards, and over the head of the observer. If, on the other hand, a deep river of wind is blowing directly towards the observer, the upper part of the front of the wave will be inclined down and towards him, concentrating the sound along the surface of the earth.

The science of acoustics in regard to the phenomena of sound as exhibited in limited spaces has been developed with signal success. The laws of its production, propagation, reflection, and refraction have been determined with much precision, so that we are enabled in most cases to explain, predict, and control the phenomena exhibited under given conditions. But in case of loud sounds and those which are propagated to a great distance, such as are to be employed as fog-signals, considerable obscurity still exists. As an illustration of this I may mention the frequent occurrence of apparently abnormal phenomena. Gen. Warren informs me that at the battle of Seven Pines, in June, 1862, near Richmond, Gen. Johnston, of the Confederate army, was within three miles of the scene of action with a force intended to attack the flank of the Northern forces, and although listening attentively for the sound of the commencement of the engagement, the battle, which was a severe one, and lasting about three hours, ended without his having heard a single gun. (See Johnston's Report.) Another case of a similar kind occurred to Gen. McClellan at the battle of Gaines' Mills, June 27, 1862, also near Richmond. Although a sharp engagement was progressing within three or four miles for four or five hours, the general and his staff were unaware of its occurrence, and when their attention was called to some feeble sound they had no idea that it was from anything more than a skirmish of little importance. (See Report of the Commission on the Conduct of the War.) A third and perhaps still more remarkable instance is given in a skirmish between a part of the 2d corps under Gen. Warren and a force of the enemy. In this case the sound of the firing was heard more distinctly at Gen. Meade's head-quarters than it was at the

head-quarters of the 2d corps itself, although the latter was about midway between the former and the point of conflict. Indeed the sound appeared so near Gen. Meade's camp that the impression was made that the enemy had gotten between it and Gen. Warren's command. In fact so many instances occurred of wrong impressions as to direction and distance derived from the sound of guns that little reliance came to be placed on these indications.

In the report of a series of experiments made under the direction of the Light House Board by Gen. Duane of the Engineer Corps is the following remark: "The most perplexing difficulty arises from the fact that the fog-signal often appears to be surrounded by a belt varying in radius from one to one and a half miles. Thus in moving directly from a station the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time."

Again, in a series of experiments at which Sir Frederic Arrow and Captain Webb, of the Trinity Board, assisted, it was found that in passing in the rear of the opposite side of an island in front of which a fog-signal was pla ed, the sound entirely disappeared, but by going further off to the distance of two or three miles it reappeared in full force, even with a large island intervening. Again, from the experiments made under the immediate direction of the present chairman of the Light House Board, with the assistance of Admiral Powell and Mr. Lederle, the Light House Engineer, and also from separate experiments made by Gen. Duane, it appears that while a reflector, in the focus of which a steam whistle or ordinary bell is placed, reinforces the sound for a short distance, it produces little or no effect at the distance of two or three miles, and, indeed, the instrument can be as well heard in still air at the distance of four or five miles in the line of the axis of the reflector, whether the ear be placed before or behind it. From these results we would iner that the lateral divergency of sound, or its tendency to spread laterally as it passes from its source, is much greater than has been supposed from experiments on a small scale. The idea we wish to convey by this is that a beam of sound issuing through an orifice, although at first proceeding, like a beam of light in parallel rays, soon begins to diverge and spread out into a cone, and at a sufficient distance may include even the entire horizon.

We may mention also in this connection that from the general fact expressed by the divergence of the rays of sound, the application of reflection as a means of reinforcing sound must in a considerable degree of necessity be a failure.

By the application of the principle we have stated and the effect of the wind in connection with the peculiarities of the topography of a region and the position of the sounding body, we think that not

only may most of the phenomena we have just mentioned be accounted for, but also that other abnormal effects may be anticipated.

In critically examining the position of the sounding body in the experiment we have mentioned, in which Sir Frederic Arrow and Captain Webb assisted, it was found that the signal was placed on the side of a bank with a large house directly in the rear, the roof of which tended to deflect the sound upwards so as to produce in the rear a shadow, but on account of the divergency of the beam this shadow vanished at the distance of a mile and a half or two miles, and at the distance of say three miles the sound of the instrument was distinctly heard. I doubt not that, on examination, all the cases mentioned by General Duane, with one exception, might be referred to the same principle, the exception being expressed in the following remarkable statement in his report to the Light House Board: "The fog-signals have frequently been heard at a distance of *twenty* miles and as frequently cannot be heard at the distance of *two* miles, and with no perceptible difference in the state of the atmosphere. The signal is often heard at a greater distance in one direction, while in another it will be scarcely audible at the distance of a mile. For example, the whistle at Cape Elizabeth can always be distinctly heard in Portland—a distance of nine miles—during a heavy northeast snow storm, the wind blowing a gale nearly from Portland towards the whistle."

This is so abnormal a case, and so contrary to generally received opinion, that I hesitated to have it published under the authority of the Board until it could be verified and more thoroughly examined. In all the observations that have been made under my immediate supervision, the sound has always been heard further *with* the wind than against it. It would appear, therefore, from all the observations that the normal effect of the wind is to diminish the sound in blowing directly against it.

There is, however, a meteorological condition of the atmosphere during a northeast storm on our coast which appears to me to have a direct bearing on the phenomenon in question. It is this: that, while a violent wind is blowing from the northeast into the interior of the country, a wind of equal intensity is blowing in an opposite direction at an elevation of a mile or two. This is shown by the rapidly eastwardly motion of the upper clouds as occasionally seen through breaks in the lower.

As a further illustration of this principle I may mention that on one occasion (in 1855) I started, on my way to Boston from Albany, in the morning of a clear day with a westerly wind. The weather continued clear and pleasant until after passing the Connecticut River, and until within fifty miles of Boston. We then encountered a storm of wind and rain which continued until we reached the city. On inquiry I learned that the storm had commenced in Boston the evening before, and, although the wind

had been blowing violently towards Albany for *twenty* hours, it had not reached inwardly more than *fifty* miles. At this point it met the *west* wind and was turned back above in almost a parallel current. This is the general character of northeast storms along our coast, as shown by Mr. Espy, and is directly applicable to the phenomenon mentioned by General Duane, and which, from the frequency with which he has witnessed the occurrence, we must accept as a fact, though by no means a general one applicable to all stations. While a violent wind was blowing towards his place of observation from Cape Elizabeth, at the surface of the earth, a parallel current of air was flowing above with equal or greater velocity in the opposite direction. The effect of the latter would be to increase the velocity of the upper part of the wave of sound, and of the former to diminish it; the result of the two being to incline the front of the wave of sound towards the observer, or to throw it down towards the earth, thus rendering the distant signal audible under these conditions when otherwise it could not be heard. I think it is probable that the same principle applies in other cases to the abnormal propagation of sound.

For the production of a sound of sufficient power to serve as a fog-signal, bells, gongs, etc., are too feeble except in special cases where the warning required is to be heard only at a small distance. After much experience the Light House Board has adopted, for first class signals, instruments actuated by steam or hot-air engines, and such only as depend upon the principle of resonance, or the enforcement of sound by a series of recurring echoes in resounding cavities.

Of these there are three varieties. First the steam whistle, of which the part called the bell is a resounding cavity, the sound it emits having no relation to the material of which it is composed; one of the same form and of equal size of wood produces an effect identical with that from one of metal. Another variety is the fog trumpet, which consists of a trumpet of wood or metal actuated by a reed like that of a flageolet. The third variety is called the siren trumpet, which consists of a hollow drum into one head of which is inserted a pipe from a steam boiler, while in the other head a number of holes are pierced which are alternately opened and shut by a revolving plate having an equal number of holes through it. This drum is placed at the mouth of a large trumpet. The sound is produced by the series of impulses given to the air by the opening and shutting of the orifices and consequent rushing out at intervals with explosive violence of the steam, or condensed air. The instrument, as originally invented by Craignard De la Tour of France, was used simply in experiments in physics to determine the pitch of sound; but Mr. Brown of New York, after adding a trumpet to it, and modifying the openings

in the head of the drum and the revolving plate, offered it to the Light House Board as a fog-signal, and, as such, it has been found the most powerful ever employed.

In ascertaining the penetrating power of different fog-signals, I have used with entire success an instrument of which the following is a description. A trumpet of ordinary tinned iron of about three feet in length, and nine inches in diameter at the larger end and about an inch at the smaller, is gradually bent so that the axis of the smaller part is at right angles to the axis of the larger end; on the smaller end is soldered a cone of which the larger end is about two inches in diameter. Across the mouth of this cone is stretched a piece of goldbeater's skin. When the instrument is used, the opening on the larger end is held before the instrument to be tested, the membrane being horizontal, and the mouth of the trumpet vertical; over the membrane is strewed a small quantity of fine sand which is defended from the agitation of the air by a cylinder of glass, the upper end of which is closed by a lens. When the instrument under examination is sounded, the sand being sufficiently near is agitated, it is then moved further off step by step until the agitation just ceases; this distance being measured is taken as the relative penetrating power of the sounding instrument. The same process is repeated with another sounding instrument, and the distance at which the sound ceases to produce an effect on the sand is taken as the measure of the penetrating power of this instrument, and so on. On comparing the results given by this instrument with those obtained by the ear on going out a sufficient distance, the two are found to agree precisely in their indications. The great advantage in using this contrivance is that the relative penetrating power of two instruments may be obtained within a distance of a few hundred yards, while to compare the relative power of two fog-signals by the ear requires the aid of a steamer and a departure from the origin of sound in some cases of fifteen or twenty miles.

X.

BIOGRAPHICAL NOTICE OF ARCHIBALD ROBERTSON MARVINE.

BY J. W. POWELL.

(READ JUNE 3, 1876.)

Mr. ARCHIBALD R. MARVINE was born at Auburn, New York, Sept. 26, 1848. When a youth he attended the military school at Sing Sing, and subsequently the School of Technology at Philadelphia. Leaving the latter he entered the Hooper Mining School of Harvard University, from which he graduated in 1870, when he was appointed instructor in the same school, a position which he held until July, 1871.

In 1869 Professor J. D. WHITNEY, of Harvard University, conducted a party of students on a trip among the Park Mountains of Colorado for the purpose of making practical studies in geography and geology. On this trip Mr. MARVINE, who was one of the party, was instructed in both these branches. The field of study was happily selected, being a portion of the Park Mountain region including South Park and the lofty mountains with which it is walled. The geographic features of the region in its larger outlines are simple. On the north is the great Colorado Range, a wilderness of crags and peaks; on the south the great group of mountains of which Pike's Peak is the culmination; on the east the Front Range, a low, broad, flat-top mountain mass; on the west the Park Range.

The Colorado Range along its main course west of Denver has a north and south axis, but near its southern end it sweeps westward in a great curve and abuts against the Park Range, and this southern curve forms the boundary between North and Middle Parks. Just here where the Park and Colorado Ranges unite, South Platte River heads in snow banks that even midsummer suns cannot destroy, and running eastward at the foot of the Colorado Range, it breaks through the Front Range by a deep, narrow cañon set with crags and pinnacles. In the midst of the Park are low subsidiary ranges chiefly trending north and south, and between these ranges there are long, narrow valleys heading in the Pike's Peak Mountains and stretching northward to the foot of the great Colorado Range, where the streams that meander through these valleys yield their waters to the South Platte.

The party climbed Pike's Peak, and, standing on its summit, the district of country just described—South Park with its mountain walls, its interior ranges, its deep valleys, its wild gorges, its silver rivers and its crystal lakes, was all before them in one grand view. In such a field the young men were instructed in determining latitudes and longitudes, in determining relative positions by triangulation, and in delineating the more important topographic features.

But mountains and valleys have something more than positions and magnitudes to interest the student of nature—they have structure; and in this structure are revealed the great facts of geology relating to displacement, degradation, sedimentation, metamorphism, and extravasation; and the field of study presented many interesting facts in each of these categories of phenomena. The great ranges stood before them to attest to the displacement or corrugation of the Earth's surface in mountain wrinkles, and these mountains are seen to be but residuary fragments of upheaved masses as plainly giving evidence of degradation as they do of displacement; while the methods of degradation by the wash of rains, by the sapping of cliffs, by the corrosion of water channels, and by the sculpture of ice, could be studied on every hand.

To MARVINE, the summer's study was rich in results. He learned that a mountain was more than a mountain, that it was a fragment of earth's history.

In the summer of 1870 Mr. MARVINE was appointed assistant geologist to attend the celebrated Santo Domingo Expedition, and on his return he prepared a brief report on the economic geology of some portions of that island which he visited. This was published with the report of the Commission of Inquiry to Santo Domingo by authority of Congress. It was a special and brief study, and contains little of general interest to the geologist.

In 1871 he received an appointment as astronomer to the Wheeler Expedition, in which capacity he served for several months, and then continued several months longer as geologist. His report on the geology of a district of country through which he passed, embraced in southern Nevada, northwestern Arizona, and southern California, has lately been published as one of the papers in Volume III. of the United States Engineer Reports of the Explorations and Surveys west of the 100th Meridian, Lieut. GEORGE M. WHEELER in charge. The region of country studied by Mr. MARVINE during these months of field service was one of great complexity. On one side of his general route of travel an extensive series of sedimentary formations are revealed, embracing Tertiary, Mesozoic, and Paleozoic groups; on the other, low mountain ranges are seen rising from a desert sea of sand, the ranges being composed of more ancient sediments and schists. The former is a portion of the Plateau Province, the

latter a portion of the Desert Province. Both the Plateau sediments and the Desert Range rocks are greatly masked by beds of basalt and other extravasated material. Back and forth across the zone separating the plateaus from the Basin Ranges he passed in long rapid marches, studying now the one, now the other region, and ever examining the volcanic phenomena by which he was surrounded, and yet his keen and well-trained eye caught the more important topographic and geologic features, and he has given in his report a singularly clear and comprehensive account of the region when we consider the circumstances under which it was made. He recognized the principal structural characteristics of the plateaus, and in some instances the structure of the Basin Ranges. He recognized that he was travelling on the border between the two. He discovered the sequence of the sedimentary groups of the Plateau Province, and collected sufficient paleontological evidence to demonstrate his conclusions. He closes this report with the following characteristic and vivid description of the Desert Range Province: "It is a great depressed mountainous region, deeply buried beneath the sediments which have been eroded from its own mountains by a surrounding sea. This action has filled the valleys, gradually covered the foot-hills, and, removing the *débris* from the mountain bases as fast as formed, has left their clean and sharp-cut tops projecting above the surrounding plain without the usual accompaniment of foot-hills and border region which surround nearly all ranges, the changes on the contrary from mountain slope to the gentle incline of the plain being generally very abrupt. The mountains seem to be of ancient plutonic or metamorphic rocks, or else of lavas; the former more often forming ranges, of which the majority trend about northwest and southeast; the latter more frequently occurring as striking isolated peaks. The detrital filling varies from gravels traceable to the rocks of adjacent hills, to the finest of alluvium, the dust of which the winds often carry for miles into Northern Arizona. It is sparsely sprinkled with a dreary vegetation, composed principally of scattered individuals of many species of mimosæ and of cacti, the most striking of the latter being the tall and isolated *Cereus giganteus*.

"To stand on the edge of the Piñal Mountains upon a quiet day and look off upon these wonderfully silent and arid plains, with their innumerable 'lost mountains' rising like precipitous islands from the sea, all bathed in most delicate tints, and lying deathlike in the peculiar, intangible afternoon haze of this region, which seems to magnify distant details rather than to subdue them, impresses one most deeply. The wonderful monotony seems uninclosable by an horizon; and one imagines the scene to continue on the same and have no end. Though the gulf and ocean are three hundred miles away, yet here is the continent's real southwestern border.

"Were the waters of the Gulf of California suddenly changed to gravel and sand, with its precipitous and rugged mountain islands left projecting from the surface as now, there would be so produced an excellent representation of these deserts, and, geologically speaking, it is but as yesterday that precisely the same action was going on over all this enormous area as is now in progress in the more confined region of the Gulf. The slow elevation which has in part probably caused the gradual receding of the waters, may still be extending the area of our continent."

Up to this time Mr. MARVINE's geological studies had been somewhat general and desultory—necessarily so from the conditions under which they were made. But in the following summer he was engaged with Professor PUMPELLY in the Keweenaw Copper Region on the shore of Lake Superior, and his report on this work was published by authority of the Legislature of Michigan.

His work during this season was confined to a narrow area, and was special, and is a fine example of painstaking, minute geological study. It consisted in tracing a series of geological beds through two or three counties lying along the lake shore. This was done by careful triangulation and levelling of the general area, and the following of the dips and strikes of the beds and measuring their thicknesses, and by carefully analyzing their lithologic and mineralogic constituents. The entire work is presented in a series of sections and tables carefully and skilfully arranged, with a general discussion, sufficiently elaborate to set forth the relations of all the important facts. His work is thus a fine model for what must be done throughout the Lake Superior region before any general discussion of the geology of the district can be made which will have permanent value.

Mr. MARVINE thus demonstrated that his experience in the fragmentary work incident to a geological reconnoissance had not led to such habits of hasty conclusion as to incapacitate him for the more thorough work of a geological survey.

Yet up to this time his work was but fragmentary, but in March, 1873, he was given a position as geologist in the corps of the U. S. Geological and Geographical Survey of the Territories under Dr. HAYDEN.

During the following season his field of research embraced the region of Middle Park in Colorado Territory, including the mountains by which it is inclosed, and extending eastward to the Great Plains, embracing an area of about 5000 square miles. How thoroughly his work was done, how clearly the geography and geology of the region was set forth in his report, and what important conclusions he reached in mountain structure and geological history, can only be fully appreciated by a careful and thorough reading of his report. It is impossible to understand a discussion of the geological structure of a region without first fully grasping the character and magnitude of its geographic

features. Geology is revealed in topography. The details of topography may seem simple, and taken severally may be simple, but in groups they become extremely complex, and few persons readily comprehend the order and system with which topographic features are gathered about the great geological structure lines of a region. It is easy to be lost in a maze of hills and a confusion of mountain peaks unless the grand topographic forms on which the hills and mountains are sculptured are seen with a mental vision that reaches further than the eye. He who can see a mountain range, or a river drainage, or a flock of hills, is more rare than a poet. In anatomy there is a place for apophysis and sinus, for arch and foramen; so in a mountain range there is a place for peak and valley, a place for amphitheatre and cañon, and the geologist who seeks to reveal the embryology and growth of a mountain range must first become thoroughly familiar with its anatomy. A hill may be a hundred or five hundred feet high, a mountain a thousand or ten thousand feet in altitude, and these may be interesting facts, but they give no clue to hill or mountain structure, and have values of the same order as the size of animals in systematic zoölogy. Not every geologist has been able to understand the geography of a region studied, and very few indeed have been able to describe the geography of a district. Something more is needed than to make mention of mountains and hills, of valleys and cañons; the order in which they are arranged must be set forth, and their relations to the general structure must be explained.

Mr. MARVINE went into a region which to the common eye would seem but a wild confusion of mountains and valleys, of crags and gorges, but in that single summer's study he discovered the sublime order in which the mountains and hills and ridges were placed, and in the first few pages of his report he sets forth this order in language clear and simple, giving a plain bird's-eye view of all that five thousand square miles of mountain crag and cañon gorge. Then he divides the area into three natural geographic divisions, and hence geologically distinct; the zone of ridges separating the plains from the mountains or mountain border region; the great range and Middle Park. In the first he found a series of sedimentary groups having a total thickness of more than 7000 feet, and a natural grouping was first discovered; then he studied the overlaps and out-thinnings, the changes in conditions of sedimentation, the grand displacements due to orographic movements, and the minor concomitant flexures and faults. All of these facts he presents in orderly arrangement with appropriate diagrams and sections. His chapter on this topic is full of facts and yet it never wearies the reader, for every fact has a meaning. The geological literature of America is greatly burdened with inconsequent facts: A geologist repairs to the field, finds a sandstone, measures it and it is ninety-nine

feet in thickness; the next day he finds a limestone, measures it and it is a hundred and one feet in thickness. He returns and reports, and his report has the same value as that of the zoölogist who went into the woods and found an animal with four legs and a tail, and the tail was four inches long as determined by careful estimation or barometric measurement. But the thickness of the limestone or the length of the animal's tail are facts of very little value except as related to those of greater significance.

The geological report which has no reference to geological structure is dreary reading, and less interesting as a recreation than a table of logarithms; while the latter has a logical arrangement and may serve some important purpose, and the student may find a meaning in the figures, the former is purposeless and meaningless. Some of our geological literature could be burned and no harm done. O that a pope would rise in the holy catholic church of geologists—a pope with will to issue a bull for the burning of all geological literature unsanctified by geological meaning. Then there would remain the writings of those inspired with the knowledge that a mountain has structure, that every hill has an appointed place and every river runs in a channel foreordained by earth's evolution, and MARVINE's work would be a book of genesis in the bible of the geological priesthood. To those members of the Society who have not made a special study of American geology and its literature, this statement may seem an exaggerated panegyric; but let him wade by months and years of study through the volumes of valueless records by which geological literature is encumbered and then take up MARVINE's paper on the Middle Park district and his appreciation will be meagre, his enthusiasm cold if he does not exclaim that order has moved on chaos.

In his third chapter Mr. MARVINE discusses the structure of the great Colorado Range. Two great facts appear: first, that the range is composed of metamorphic schists and granites having a detailed structure independent of the grand topographic forms now existing, but related to a topography antecedent to the present and which was buried by encroaching waters prior to the upheaval of what we now know as the great Colorado Range; and, second, that the great orographic movements producing the present grand features of the country brought up once more that ancient and buried land; and the present drainage system, determined by these later upheavals, while conforming to the later structure, was superimposed on the earlier; and his facts are assembled in such manner in this chapter that his grand conclusions are fully demonstrated.

His fourth chapter is on the Middle Park proper. This is an exceedingly complex piece of geology, and to properly characterize the chapter it would be necessary to substantially reproduce

it. The vestiges of earth's history found here where the sea and clouds have alternate dominion over the land, are set forth in a manner simple and perspicuous.

One very important conclusion reached by Mr. MARVINE must not be neglected, viz., that the ranges about this park were not upheaved as great appressed folds, but that the upheaval was along lines by faulting, or narrow zones by abrupt flexure—an important characteristic of displacement throughout much of the interior of this continent—and these facts are eventually destined to modify if not revolutionize the geological theories concerning the constitution of the earth.

After the preparation of this report, in the spring of 1874, Mr. MARVINE again returned to Colorado Territory for the purpose of extending his geological studies in the region west of Middle Park. From my intimacy with Mr. MARVINE I know well with what eagerness he resumed these studies and how anxious he was to pursue lines of investigation suggested by facts discovered in his previous work. And so, fired with an enthusiasm for the discovery of the secrets of the mountains, he plunged into the wilderness far away from civilization. All that summer long he toiled, climbing only where the geologist would climb, seeing only what the geologist could see; and still eager for more knowledge, he pressed his work until the desolate mountains were mantled with the winter's snow, and a further study of geology was impossible; then he returned. But the labor and hardships of the summer's travel, though unheeded at the time, were too great for his physical endurance, and on his return he was prostrated with the disease that held him in firm grasp for many long weeks. Slowly during the following spring he partially recovered, and then, although he was not able to work with vigor, those with whom he was more intimate and who loved to talk with him on the subjects of his investigation, learned the great results and significance of the past year's study. Not recovering health and strength, he was unable to return to the field or to prepare the results of his former work for publication; still he worked on his map, coloring it for the purpose of showing the geographic distribution of the geological formations within his field of study, and this was done with elaboration. Then he thoroughly arranged and systematized his notes and determined his plan of discussion. Here his work ended, for health and strength failed again, and he relapsed into a condition that his friends soon found was hopeless. On the second of March, in the city of Washington, MARVINE, the young, enthusiastic, and brilliant geologist, died.

Mr. MARVINE's preparation for work as a geologist was very thorough, and for one of his age, very broad. In chemistry, astronomy, and physics, his studies had been careful and thorough, and his grasp of these subjects was comprehensive and firm; but

he was especially attracted to chemical physics, and had he lived to continue his labors as a geologist, his predilection for these studies would doubtless have greatly modified all his geological investigations.

Personally and socially his modesty was great, and this trait of character is evinced in his writings; and those who knew him intimately loved him for his honesty, a trait of character that appeared everywhere in his collection of facts and in every step made toward conclusions, and he leaves behind many to mourn the loss of his genial presence, and the labors of his vigorous and comprehensive mind.

INDEX TO NAMES OF CONTRIBUTORS.

[iii.]

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON, D.C.

VOL. III.

NOVEMBER 9TH, 1878—JUNE 19TH, 1880.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON.
1878–1880.

CONTENTS.

CONSTITUTION, STANDING RULES,

AND

LIST OF MEMBERS

OF

THE PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

April, 1879.

CONSTITUTION

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all business.

ARTICLE VI. This Constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

NOVEMBER, 1877.

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. The Annual Meeting for the election of officers shall be the first stated meeting in the month of November. When necessary, Special Meetings may be called by the President.

3. Notices of the time and place of meetings shall be sent to each member by one of the Secretaries.

4. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

5. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

6. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

7. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

8. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

9. New members shall be elected by the General Committee, after having been proposed in writing by at least three members of the Society.

10. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin. The names of those two years in arrears will be dropped from the list of members. Notice of resignation of membership should be given in writing to the General Committee through the President or one of the Secretaries.

11. The fiscal year terminates with the 31st of December of each year. Members elected after the annual meeting shall be exempt from the assessment for that year.

12. Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments, in which case their names shall be dropped from the list of members. They can, however, on returning to the District, resume their membership by giving written notice to the General Committee through the President or one of the Secretaries of their wish to do so.

13. Elections of officers are to be held as follows:—

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz, four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot so many names as there are persons yet to be elected; and

those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

14. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed to the furtherance of only such special scientific work as may be ordered by the General Committee.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

NOVEMBER, 1877.

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee and of the Society and the election of members, six members of the Committee shall constitute a quorum.

7. Proposals of new members may be read at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concurrence of twelve of the members shall be necessary to election.

8. These Standing Rules, and those for the government of the Society, shall only be modified with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

1. The President's annual address will be published in full.

2. When directed by the General Committee, any communication may be published in full in an appendix to the proceedings of any meeting. Proofs will be sent to the author when it is convenient to do so, and a number of extra copies, not exceeding fifty, will be supplied to him, if applied for before the communication is printed.

3. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and

approved by the Committee on Publications may also be published.

4. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with reference to the place of publication, if made known in season to the Committee on Publications.

NOTE.—*The attention of members is specially requested to the 3d and 4th of the above rules. If those who present communications or remarks will promptly furnish abstracts or the papers in full, the Bulletin will be more complete, and can be published with less delay.*

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

APRIL 2, 1879.

Cleveland Abbe
Sylvanus Thayer Abert
Asa O. Aldis
Benjamin Alvord
Thomas Antisell

Orville Elias Babcock
Spencer Fullerton Baird
Marcus Baker
George Bancroft
Joseph K. Barnes
Thomas W. Bartley
Henry Hobart Bates
Alexander Graham Bell
Stephen Vincent Benét
Emil Bessels
John Shaw Billings
William Birney
Rogers Birnie
Swan M. Burnett
Samuel Clagett Busey

Horace Capron
Thomas Lincoln Casey
John White Chickering
Edward Clark
John Huntington Crane Coffin
Elliott Coues

Robert Craig
Charles Henry Crane
Josiah Curtis
Richard Dominicus Cutts

William Healey Dall
George Dewey
Myrick Hascall Doolittle
Henry Harrison Chase Dunwoody
Clarence Edward Dutton

John Robie Eastman
John Eaton
Ezekiel Brown Elliott
Frederic Miller Endlich
Charles Ewing

Edward Jessop Farquhar
William Ferrel
Robert Fletcher
Edgar Frisby
Edward T. Fristoe

Leonard Dunnell Gale
Edward Miner Gallaudet
Henry Gannett
Alexander Y. P. Garnett
Grove Karl Gilbert
Theodore Nicholas Gill
William Whiting Goddard
George Brown Goode
Edward Goodfellow
Henry Goodfellow
Walter Hayden Graves
Bernard Richardson Green
Francis Mathews Green
Francis Vinton Greene
Benjamin Franklin Greene
Francis Mackall Gunnell

PETER CONOVER HAINS
ASAPH HALL
WILLIAM HARKNESS
FERDINAND VANDEVEER HAYDEN
HENRY WETHERBEE HENSHAW
JULIUS ERASMUS HILGARD
GEORGE WILLIAM HILL
EDWARD SINGLETON HOLDEN
WILLIAM HENRY HOLMES
FRANKLIN BENJAMIN HOUGH
HENRY W. HOWGATE
DAVID LOWE HUNTINGTON

THORNTON ALEXANDER JENKINS
ARNOLD BURGESS JOHNSON
JOSEPH TABOR JOHNSON
WILLIAM WARING JOHNSTON

ALBERT FREEMAN AFRICANUS KING
JOHN JAY KNOX

JONATHAN HOMER LANE
WILLIAM LEE
NATHAN SMITH LINCOLN
EDWARD PHELPS LULL

GARRICK MALLERY
JOSEPH BADGER MARVIN
OTIS TUFTON MASON
FREDERIC BAUDERS MCGUIRE
WILLIAM MCMURTRIE
MONTGOMERY CUNNINGHAM MEIGS
MONTGOMERY MEIGS
ANICETO GABRIEL MENOCAL
WILLIAM MANUEL MEW
MARTIN FERDINAND MORRIS
ALBERT J. MYER

SIMON NEWCOMB
WALTER LAMB NICHOLSON

John Walter Osborne
George Alexander Otis

Robert Lawrence Packard
John Grubb Parke
Peter Parker
Henry Martyn Paul
Albert Charles Peale
Orlando Metcalfe Poe
John Wesley Powell
Henry Smith Pritchett

Charles Valentine Riley
John Rodgers

Benjamin Franklin Sands
James Hamilton Saville
Charles Anthony Schott
Henry Robinson Searle
Samuel Shellabarger
John Sherman
William Tecumseh Sherman
Charles Dwight Sigsbee
Aaron Nichols Skinner
David Smith
Ainsworth Rand Spofford

William Bower Taylor
Almon Harris Thompson
David P. Todd
Joseph Meredith Toner
William J. Twining

Jacob Kendrick Upton

George Vasey

Lester Frank Ward
James Clarke Welling
George M. Wheeler

CHARLES ABIATHAR WHITE
ALLEN D. WILSON
JAMES ORMOND WILSON
JOSEPH JANVIER WOODWARD

HENRY CRISSEY YARROW

ANTON ZUMBROCK

Members who have been absent from the District of Columbia for more than twelve months, and who may on their return resume their membership by giving written notice to the General Committee through the President or one of the Secretaries of their wish to do so.

LESTER A. BEARDSLEE

AUGUSTUS LUDLOW CASE
FRANK WIGGLESWORTH CLARKE

RICHARD CRAIN DEAN

HUGH EWING

ELISHA FOOTE

JAMES TERRY GARDNER
EDWARD OZIEL GRAVES

ISAIAH HANSCOM
EDWIN EUGENE HOWELL

HENRY ARUNDEL LAMBE JACKSON

REUEL KEITH

WILLIAM MYERS

CHARLES HENRY NICHOLS

Charles Christopher Parry
Titian Ramsay Peale
Benjamin Peirce
Charles Sanders Peirce

Henry Reed Rathbone
Christopher Raymond Perry Rodgers
Joseph Addison Rogers

Montgomery Sicard
John Stearns
Ormond Stone

William Calvin Tilden

Francis Amasa Walker
Junius B. Wheeler
Joseph Wood
William Maxwell Wood
Christopher Columbus Wolcott

Deceased Members.

Theodorus Bailey

Salmon Portland Chase
Benjamin Faneuil Craig

Charles Henry Davis
Frederic William Dorr
Alexander B. Dyer

Amos Beebe Eaton

John Gray Foster

Joseph Henry

F. Kampf

OSCAR A. MACK
ARCHIBALD ROBERTSON MARVINE
FIELDING BRADFORD MEEK

JOHN CAMPBELL RILEY

GEORGE CHRISTIAN SCHAEFFER

JOHN MAYNARD WOODWORTH

MORDECAI YARNALL

Members are requested to give notice to one of the Secretaries of any error in their names, also of any error or change of address, or of intention of being absent from the District of Columbia more than twelve months and wish on account thereof to be excused from payment of the annual assessment. (*Art. 12 of the Standing Rules.*)

BULLETIN

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

149TH MEETING. 8TH ANNUAL MEETING, NOVEMBER 9, 1878.

Vice-President TAYLOR in the Chair.

Thirty-five members present.

The names of members elected since the last annual meeting were read.

The election of officers for the ensuing year was conducted in accordance with the rules, with the following result:—

President,	SIMON NEWCOMB.	
Vice-Presidents,	J. K. BARNES,	W. B. TAYLOR,
	J. E. HILGARD,	J. C. WELLING.
Treasurer,	CLEVELAND ABBE.	
Secretaries.	J. H. C. COFFIN,	T. N. GILL.

MEMBERS OF THE GENERAL COMMITTEE.

THOMAS ANTISELL,	C. E. DUTTON,
E. B. ELLIOTT,	J. W. POWELL,
ASAPH HALL,	C. A. SCHOTT,
W. HARKNESS,	J. M. TONER,

J. J. WOODWARD.

150TH MEETING. NOVEMBER 23, 1878.

The President, Mr. NEWCOMB, in the Chair.

Forty-five members and visitors present.

The minutes of the last meeting were read and adopted.

The election of Maj. WILLIAM J. TWINING, of U. S. Engineers, and Mr. DAVID P. TODD, of the Nautical Almanac Office, as members of the Society, was announced.

A paper by Mr. E. S HOLDEN, entitled

NOTES ON THE BRIGHTNESS AND THE STELLAR MAGNITUDE OF THE THIRD SATURNIAN SATELLITE, TETHYS,

was read by Mr. SKINNER.

Remarks were made by Mr. ABBE.

Mr. J. J. WOODWARD made remarks

ON THE APERTOMETER OF PROF. E. ABBE, OF JENA, GERMANY.

(ABSTRACT.)

Mr. WOODWARD exhibited and described the apertometer devised by Professor Abbe, of Jena, for measuring the aperture of microscopic objectives, and manufactured by Carl Zeiss, also of Jena. An account of this instrument will be found in the Journal of the Royal Microscopical Society, March, 1878, p. 19. As the instrument was designed for use on the perpendicular microscope stands commonly employed in Germany, Mr. Woodward had modified it by mounting the glass disk on a block of wood 3 inches long by 1.5 wide and .4 thick, and by attaching springs of thin sheet brass to the two movable indices, the object being to allow the apparatus to be used on English or American stands inclined at convenient angles. He praised the ingenuity of Professor Abbe's device and the excellence of the workmanship of Zeiss, and commended the instrument as affording a convenient method of measuring the apertures of objectives by lamplight. He still preferred, however, as simpler and more convenient, the method he had used for several years in measuring such apertures by sunlight. As originally used, this method was described by him in the *Monthly Microscopical Journal*, June, 1873, p. 268. The objective was screwed to an opening in the shutter of the dark room; a parallel pencil of solar light was thrown through it from behind by a plane mirror, while the front lens of the objective was in contact with a thin sheet of Canada balsam confined between two plates of glass, one of them ground. The solar rays, after coming to a focus in the balsam, diverged, and the margins of the cone of light could readily be marked with a lead-pencil on the surface of the plate of ground glass. The angle formed was afterwards measured with a protractor, which gave

the "balsam angle" of the objective. Subsequently, at the suggestion of Mr. Tolles, of Boston, he substituted for the sheet of balsam a quadrangle of crown-glass ground on one side and polished on the edges. One of the edges was placed in front of the objective with which it was connected with the immersion fluid for which the objective was intended. The solar rays coming from behind, after passing through the crown-glass front of the objective, and suffering refraction on entering and leaving the immersion fluid, resumed in the quadrangle of crown-glass the direction they had in the crown-glass front. The margins of the visible cone were marked on the ground glass surface of the quadrangle with a lead-pencil, and the angle read by a protractor. The angle thus measured was of course substantially the angle of the extreme rays in the crown-glass front. It might be conveniently called the "interior angle" of the objective, with which it would be identical were it not for trifling differences between the refractive index of the crown-glass quadrangle and that of the crown-glass selected for the front of particular objectives. Measured in this way it was easy to see experimentally that the limit of 82° interior angle, which Mr. Wenham had assigned to all objectives, did not practically exist. He himself, Professor R. Keith, and lately Professor Stokes, of Cambridge, had shown that the reasoning on which Mr. Wenham based his assertion was not in accordance with optical theory. Experimental observation indicates that the number of makers who produce objectives with more than 82° interior angle is increasing. This was the case in this country, especially with many of the immersion objectives of Tolles and Spencer; in England, with some of the immersion objectives of Powell and Lealand, and on the Continent with the immersion objectives of Zeiss. From the latter maker he had recently received two of his new oil-immersion objectives, a $\frac{1}{8}$th and a $\frac{1}{12}$th. As measured by the glass quadrangle by sunlight, the $\frac{1}{8}$th had an interior angle of 115°, the $\frac{1}{12}$th of 114°. Measured by the apertometer of Abbe the results were, as nearly as could be estimated, identical. Both objectives exceeded a little the number 1.25 on Abbe's arbitrary scale, which corresponds to 113° interior angle; but owing to the character of that scale the exact amount of excess could only be estimated. Mr. Woodward regarded the adoption of that scale as an unfortunate one. A simple division of the glass circle into degrees would have been in many respects more convenient.

Mr. J. E. HILGARD made a communication on

JABLOKOFF'S ELECTRIC CANDLE.

Mr. W. H. DALL commenced a paper entitled

NOTES ON THE MUSEUMS AND ZOOLOGICAL GARDENS OF NORTHERN EUROPE.

151st Meeting. December 7, 1878.

The President in the Chair.

Fifty-one members and visitors present.

The election of Mr. John Walter Osborne as a member of the Society was announced.

Mr. Henry Reed read a paper on

THE PHYSIOLOGY OF CIVILIZATION.

Remarks were made by Messrs. Woodward, Powell, Harkness, Hayden, Newcomb, White, Chipman, Dutton, Alvord, Gill, J. D. Cox, Elliott, Mason, Parker, and Dall, generally controverting views advanced by Mr. Reed.

Mr. Asaph Hall made the following communication

ON THE SUPPOSED DISCOVERY OF A TRANS-NEPTUNIAN PLANET AT THE U. S. NAVAL OBSERVATORY IN 1850.

In October, 1850, Mr. James Ferguson, Assistant Astronomer of the Naval Observatory, while observing the planet Hygeia, observed on four nights an object of the 9½ magnitude which afterwards seemed to have disappeared. A communication on this subject was made by the Superintendent of the Observatory, Mr. Maury, to the Secretary of the Navy, and Mr. Maury's letter, together with Mr. Ferguson's observations, were published in *Gould's Journal*, Vol. II., p. 53. Mr. Hind, of London, discussing these observations, came to the conclusion that the object observed by Mr. Ferguson was a trans-Neptunian planet, at a distance from the sun of "more than 137, and a period of above 1600 years." *Gould's Journal*, Vol. II., p. 78.

Recently Professor C. H. F. Peters, Director of the Litchfield Observatory of Hamilton College, has given this matter a critical examination, and has found that the true explanation is that Mr Ferguson made a mistake in observing the difference of declination, and that by making the proper corrections the whole series of observations comes into harmony, and the missing object proves to be a well-known fixed star.

Mr. Hall, on examining the original observing books of Mr.

Ferguson, finds that the explanation given by Prof. Peters is undoubtedly correct. These books show that Mr. Ferguson made éleven comparisons of the difference of declination, and that eight of these comparisons were correct, and three erroneous. For some unknown reason Mr. Ferguson in his reductions changed the eight correct comparisons to correspond with the three erroneous ones, and this arbitrary change produced the mysterious pseudo-planet.

152D MEETING. DECEMBER 21, 1878.

The President in the Chair.

Mr. W. H. DALL continued his communication on

THE MUSEUMS AND ZOOLOGICAL GARDENS OF NORTHERN EUROPE,

describing those at Bremen, Berlin, Gotha, Frankfort-am-Main, Stuttgart, Bonn, Cologne, Amsterdam, and Paris; also those of London, Oxford, Cambridge (Eng.), Edinborough, Dublin, and Liverpool: also answering inquiries made by Messrs. NEWCOMB, POWELL, ELLIOTT, and HARKNESS.

Mr. J. W. OSBORNE made a communication on

SUGGESTIONS RESPECTING THE STUDY OF METEOROLOGY IN REGARD TO THE CAUSES OF YELLOW FEVER.

Remarks were made by Mr. ANTISELL.

153D MEETING. JANUARY 4, 1879.

The President in the Chair.

Twenty-three members and visitors present.

Mr. A. N. SKINNER communicated

A NOTE ON THE PRECESSION OF STARS IN RIGHT-ASCENSION.

Remarks were made by Mr. NEWCOMB.

Mr. J. J. WOODWARD made the following communication

ON A STANDARD FOR MICROMETRY.

Mr. WOODWARD read a letter he had recently received from a committee of the microscopical section of the Troy Scientific Association, asking answers to the following questions:—

"1. Is it expedient at present to adopt a standard for micrometry?

2. If so, should the English or the metric system be employed?

3. What unit, within the system selected, is most eligible?

4. What steps should be taken to obtain a suitable standard measure of this unit?

5. How can this standard micrometer be best preserved and made useful to all parties concerned?"

These questions were asked with a view to some action to be taken on the subject at the meeting of the American Society of Microscopists to be held in Buffalo during the summer of 1879. He had made the following reply, with regard to which he invited comments or criticism by the Society.

I submit the following replies to the questions of your circular letter of December 2d:—

1. I am in favor of the adoption of a suitable standard for micrometry by the American Society of Microscopists at their next meeting.

2. For this particular purpose I think the metric system offers so many conveniences that I favor its employment.

3. The selection of an eligible unit within the system involves, it appears to me, two distinct questions: *A*. How shall the stage-micrometer be ruled? *B*. How shall the measurements made be expressed in speech or writing?

A. The object of the stage-micrometer is chiefly to give values to the divisions of the eyepiece-micrometer with the power used in any given case. It should be long enough to be used for this purpose with the lowest powers of the compound microscope, and have a part of its length ruled sufficiently close to answer the same end with the highest powers. I favor the adoption of a standard scale a centimetre long ruled in millimetres, and one of these ruled in hundredths. I have used stage-micrometers ruled in thousandths of a millimetre, but regard such divisions as inconveniently close for this purpose. To measure in thou-

sandths of a millimetre as the unit, which is very convenient in a large number of cases, the simplest way is to use a magnifying power that will make ten divisions of the eyepiece-micrometer exactly coincide with one-hundredth of a millimetre on the stage-micrometer. The glass eyepiece-micrometer should have a scale a centimetre long ruled in one hundred parts. By increasing the power so that a larger number than ten of these divisions shall correspond to one-hundredth of a millimetre on the stage-micrometer, a unit of any degree of minuteness that may be required for any special work can be obtained up to the limits of distinct vision with the microscope.

B. But although I regard the hundredth of a millimetre as a very eligible dimension for the closest divisions of the stage-micrometer, when it comes to expressing the results of our measurement in speech or writing, I do not think it is convenient to use the hundredth of a millimetre as the unit of expression. It is too large, and the results of too many measurements would still have to be expressed in decimal fractions. The thousandth of a millimetre is much more convenient as a unit of expression, and I would advise that microscopists should agree to call this dimension a *micron*, and represent it in writing by the Greek letter μ. This dimension has already been adopted as the unit of expression by a number of European microscopists, who represent it by the same Greek letter, but call it a micro-millimetre. The term MICRON should, I think, be preferred because well known to scientific men other than microscopists, having for some time been used in expressing minute differences by those officially engaged in preparing standard measures of length, and having been adopted by the International Metric Commission. I think it running an unnecessary risk of confusion to select any other than this well-recognized term for the dimension in question.

4 and 5. To obtain a suitable standard stage-micrometer, I would advise each microscopical society to select one ruled as above described, by any person in whom they have confidence, and to satisfy themselves by comparison of the several parts with each other, by means of the same part of the eyepiece-micrometer, that the divisions agree among themselves. This is comparatively easily done; the real difficulty will be to determine whether the whole scale is really a centimetre long. To ascertain this, I would advise each microscopical society to send its stand-

ard micrometer to the Superintendent of the Coast Survey at Washington, with the request that he will have it compared with a recognized standard in the Bureau of Weights and Measures, and return it with a report of the error, if any. I have reason to believe that such requests would be promptly and courteously responded to. Each society should then preserve the standard thus obtained for the sole purpose of enabling its members to compare their stage-micrometers with it. I think this plan much wiser than to relegate the question to any one of the ingenious men who are endeavoring in this country, with considerable success, to make accurate rulings on glass, and I should anticipate better results from it than from the appointment of a special committee of the American Society of Microscopists to prepare a standard scale.

In conclusion, I readily admit that so long as the English microscopists continue to express the results of their measurements in decimals of an English inch, there will be American microscopists who will do the same, either for all purposes or for particular work, and of course it is very desirable that these measurements also should be accurate. The stage-micrometers on this system in the market are usually ruled in hundredths and thousandths of an inch. The latter divisions are too wide to give values to the eyepiece-micrometer with the higher powers, while the five-thousandths, ten-thousandths, or even finer divisions, ruled also on some of these micrometers, are inconveniently close. I would advise the makers to rule such micrometers four-tenths of an inch long, divided into hundredths of an inch, one of the hundredths being subdivided into ten, another into twenty-five spaces. These latter spaces, each representing one twenty-five-hundredth of an inch, sufficiently approximate the hundredth of a millimetre to be used with equal convenience with the higher powers. The scale on the glass eyepiece-micrometer, used with these stage-micrometers, should be, if specially made for the purpose, four-tenths of an inch long, divided into one hundred parts, each one two-hundred-and-fiftieth of an inch; but these divisions would so closely approximate those of the metric eyepiece-micrometer proposed, that it might be used without inconvenience instead. Where it is thought worth while by a microscopical society to procure a standard scale of this kind, it should be sent to the Coast Survey office for measurement, as in the case of the metric scales.

Remarks were made by Messrs. HARKNESS, ELLIOTT, and TAYLOR.

Mr. J. J. WOODWARD also made a communication

ON THE OIL-IMMERSION OBJECTIVES OF ZEISS, AND ON CONVENIENT METHODS OF OBTAINING OBLIQUE ILLUMINATION FOR THESE AND SIMILAR OBJECTIVES.

(ABSTRACT.)

Mr. WOODWARD exhibited and described his sub-stage prism for the illumination of balsam mounted objects for examination with immersion objectives whose balsam angle (or preferably interior angle) is 90° or upwards. It is described in the Journal of the Royal Microscopical Society, November, 1878, p. 246. By using a prism of 98° angle instead of a right-angled one, immersion objectives of less than 90° but more than 82° interior angle could be satisfactorily illuminated. Both prisms had recently been manufactured without any mounting by G. S. Woolman, 116 Fulton Street, New York. This had the advantage of economy; but the prism being necessarily attached to the under surface of the slide by glycerine or oil of cloves, would move with the object, which was inconvenient. He therefore greatly preferred the instrument as originally made, although of course the same results could be obtained by Woolman's modification, provided the prism was readjusted each time the object was moved. He also exhibited and described a more complex apparatus which he had devised for microphotography, and which enabled him to register the precise angle of the illuminating pencil employed. (This apparatus will be described in full and figured in the Journal of the Royal Microscopical Society during 1879.)

Mr. WOODWARD remarked that although the most oblique pencil an immersion objective can take in is necessary to obtain the best results on lined test objects, *e. g.*, Amphipleura pellucida mounted in Canada balsam, this was no longer the case when direct sunlight was used. In illustration he exhibited a photograph of Amphipleura pellucida taken with about 2700 diameters by the oil-immersion $\frac{1}{12}$th of Zeiss, referred to by him in his communication of November 23d, when he stated that its interior angle was 114°. The photographs excelled in definition any picture of this test he had ever obtained with any objective; and yet the optical axis of the illuminating lens (a 3-inch objective of 10° aperture which was used without any substage lens or prism) was only inclined at an angle of 45° to the optical axis of the microscope. As a further illustration of the difference between the results obtained by illuminating the microscope by lamp and sunlight, he exhibited two photographs of Pleurosigma angulatum taken by the oil-immersion $\frac{1}{8}$th of Zeiss of 115° interior angle.

One of them showed the well-known appearance of hexagons, the other the longitudinal diffraction lines produced in the experiment devised by Professor Abbe, of Jena. Professor Abbe had found that if the central part of a high-angled objective be stopped out by a transverse bar and a frustule of Pleurosigma angulatum examined, its midrib being parallel to the bar and the light thrown at right angles to it, the hexagonal markings would be no longer seen, but instead a series of sharp diffraction lines parallel to the midrib. In their distance apart these markings are so related to the distance of the hexagons from centre to centre that seven of the diffraction lines occupy the space of four of the hexagons. By lamplight, Professor Abbe found that these diffraction lines appeared only on those parts of the Angulatum frustule which were fused to the glass cover in preparing the specimen; where a film of air existed between the frustule and the cover they did not appear. Mr. WOODWARD had found that this was true only for illumination by lamp; by sunlight the diffraction lines appeared on all parts of all the frustules. In the photographs which he exhibited the boundary of the adherent part of the frustule could be distinctly seen, and the diffraction lines were equally distinct on the non-adherent parts. Professor Abbe's explanation of the phenomenon was therefore inadequate, as it required the non-appearance of the diffraction lines on the non-adherent parts by sunlight as well as by lamp.

The subject was further discussed by Messrs. HARKNESS and WOODWARD.

Mr. ASAPH HALL made a communication

ON THE SATELLITES OF SATURN.

(ABSTRACT.)

Mr. HALL stated that his observation of Hyperion, the faint satellite of Saturn, made in 1878, indicate that the line of apsides of the orbit of this satellite has a rapid motion. Probably this line has revolved at least 180° since the time of the discovery of this satellite in 1848 by the Bonds and Lassell.

Attention was called to the peculiar relations of the orbits of Hyperion and of the large satellite Titan, through which Titan is able to exert a great influence on the motion of Hyperion. The motion of the line of apsides of Hyperion will probably furnish an accurate determination of the mass of Titan.

Remarks were made by Messrs. TAYLOR and NEWCOMB.

154TH MEETING. JANUARY 18, 1879.

The President in the Chair.

Forty-one members and visitors present.

The President referred to the recent memorial services of Prof. HENRY at the Capitol, and stated that an invitation to attend them had been extended to the Philosophical Society, and that the Society was represented on the occasion.

Mr. J. W. OSBORNE gave a recapitulation of his communication made at a previous meeting, on

THE APPLICATION OF METEOROLOGY TO YELLOW FEVER INVESTIGATIONS.

Remarks were made by Messrs. ANTISELL, ABBE, and WOODWARD.

Mr. J. W. OSBORNE also made a communication on

A CURIOUS MANIFESTATION OF FORCE BY THE WIND,

exhibiting a distorted wind vane.

Mr. E. B. ELLIOTT made a communication on

THE PROGRESS OF INTERNATIONAL COINAGE IN FRANCE AND AMERICA.

155TH MEETING. FEBRUARY 1, 1879.

The President in the Chair.

Thirty-one members and visitors present.

Medical Director FRANCIS M. GUNNELL, U. S. Navy, and Mr. GEORGE WILLIAM HILL, of the Nautical Almanac Office, were announced as having been elected members of the Society.

Mr. F. M. ENDLICH made a communication

ON SOME INTERESTING CASES OF METAMORPHISM.

The subject was discussed by Messrs. ANTISELL, WHITE, and ENDLICH.

Mr. C. E. DUTTON made a communication on

THE GEOLOGICAL CHARACTER OF THE COLORADO RIVER.

Comments were made and questions asked by Messrs. WHITE, ENDLICH, and HALL.

156TH MEETING. FEBRUARY 15, 1879.

Vice-President WELLING in the Chair.

Thirty-eight members and visitors present.

The minutes of the last meeting were read and adopted.

The election of Mr. BERNARD RICHARD GREEN, Major PETER CONOVER HAINS, U. S. Engineers, Commander GEORGE DEWEY, U. S. Navy, and Mr. FREDERICK BORDEN McGUIRE, as members of the Society, was announced.

Mr. THOMAS ANTISELL read a paper entitled

OBSERVATIONS ON CHEMICAL MOLECULAR CHANGES.

Remarks were made and questions asked by Mr. HARKNESS, Prof. F. W. CLARKE, of Cincinnati, Mr. DOOLITTLE, and Mr. DALL.

157TH MEETING. MARCH 1, 1879.

Vice-President TAYLOR in the Chair.

Thirty-eight members and visitors present.

The deaths of Dr. JOHN C. RILEY and of Prof. MORDECAI YARNALL, U. S. Navy, members of the Society, were announced.

The election of Lieut. Comr. CHARLES D. SIGSBEE, U. S. N., as a member of the Society, was also announced.

Mr. S. F. BAIRD made a communication on

THE ARTIFICIAL PROPAGATION OF THE COD,

describing the measures and process adopted at Gloucester, Mass., and the success thus far obtained. A vessel is to be specially constructed for the Fish Commission, to be used for this purpose.

Mr. ELLIOTT made some remarks and inquiries respecting the extent of the cod fisheries.

Mr. P. H. DUDLEY, C. E., made a communication, illustrated by drawings, on

THE USES OF HIS DYNAGRAPH, AND THE WORK PERFORMED IN DETERMINING THE RESISTANCE OF RAILWAY TRAINS, ETC.

(ABSTRACT.)

It is an instrument designed for the purpose of determining, and graphically recording on a continuous sheet of paper, the resistance to traction of railway rolling stock of all kinds, either as single cars or in trains; also, testing the resistance to traction of locomotives of various kinds of wheel base, and also their capacity; the object being to obtain data from a knowledge of which the cost of transportation may be reduced, by substituting facts for mere opinion.

It has several attachments, so that data can be obtained to solve most of the problems connected with the movement of trains. The low joints in the rails are also shown at the same time.

The wearing effect on the rails and tires of various kinds of wheel base of locomotives can also be shown by special experiments. A car is specially constructed for its use; the present one is 50 feet long by 9 feet 6 inches wide, and has five apartments, viz.: 1st. Dynagraph room with bookcase: 2d. Sleeping room: 3d. Laboratory: 4th. Sitting room; one double overhead berth, piano, wardrobe, and washstand: 5th. Dining room, containing range, ice box, provision drawers, china cupboard, and portable table. An aisle on one side of the car permits access to any apartment. The instrument is placed near the end of the car in the Dynagraph room.

The car has two 4-wheeled trucks, each with 7 feet wheel base, all the wheels being turned truly cylindrical. The elliptical springs in the trucks are quadruplets, so that the car rides very steadily on most roads. The draw-bar is enlarged, and a steel cylinder 6 inches long and 4 inches internal diameter inserted instead of the usual fastening. In each end of the cylinder pistons with cup-leather packing are inserted and by means of annular rings both are free to move in, but prevented from moving

outward; the required movement in use is less than $\frac{1}{80}$th of an inch. The cylinder is filled with oil, and from its centre a pipe leads to a small piston on top of the instrument on the inside of the car. Any pressure on the draw-bar either pulling or pushing is transmitted to the piston (which is held by springs of known tension), and thence to arms holding the pen which records the pressure. Inside of the car is a large cast-iron frame, 36 × 40 inches, and 30 inches high, which supports the mechanism to roll the paper as the car moves, it being driven by positive gearing from the axle of the car.

The present Dynagraph uses paper 30 inches wide, though by leaving off some of the attachments, paper 20 inches wide can be used: it has gearing to represent 400, 200, 100, or 50 feet of track per inch of paper, as desired: strong Manilla paper, specially made, is used, and is wound in a continuous sheet of 400 to 500 feet in length on one of the paper drums at either end of the instrument. These drums are driven by friction and only serve to wind the paper, keeping it taut as it is fed through the machine by the steel rolls, either pair being used according to the direction which the car is running. The rolls make $1\frac{7}{10}$ revolutions for $13\frac{2}{10}$ inches of paper; this corresponds to 400 feet of track for 1 inch of paper. The rolls are $\frac{3}{1000}$ of an inch larger in diameter in the centre than at the ends.

The wheels which drive the mechanism are turned without any conning, and are 33 inches in diameter. A triple thread worm on the axle drives a shaft which is connected by three universal joints with the shafting of the instrument. There are three mitre gears so arranged with saw-tooth clutches that the paper can run in either direction, and either pair of feed rolls, also used at pleasure. Right or left-handed diagrams can be taken, as desired. There is an integrating attachment which measures constant areas of the dynamical curve and records them electrically. The electrical recording apparatus has 11 pens: one records from the clock seconds; one every tenth second, and one for each minute; one pen records the constant area on the diagram; one pen the amount of water (as measured by a meter) used by the locomotive, and where consumed; one pen the amount of coal, and its distribution; one pen the alignment of the road; one pen the distance as measured by the instrument; one pen the revolution of the drivers; one pen the velocity of the wind, and one pen the roads, mile posts, and stations.

In special experiments on trains all the pens are used as described, but some of them may be used for other purposes if desired. In making experiments on single cars we have inside the larger pistons, smaller ones, so that the same springs give five times as large a scale as with the large pistons.

Among the many important uses of the Dynagraph has been the determination of the effect of different classes of locomotive wheel bases upon the wear of rails and tires of the driving wheels.

It was found that it required more power to push the same locomotive on curves when the wheel base was so arranged that the front end of the locomotive was free to swing in a cradle of the truck (as at certain velocities, the centrifugal force would act upon the locomotive in such a manner that the flanges of the driving wheels against the rails would guide the engine; the front outside driving-wheel flange would be against the outer rail, and the rear inside driving-wheel flange against the inner rail), than it did when the same locomotive was so arranged that the front truck and the rear driving-wheel flange guided the engine. This is seen practically in its effect upon the wear of rails on different roads; the outer rail of curves on the B. & O. R. R., P. W. & B. R. R., N. Y. & H. R. R., N. Y. C. & H. R. R. R. being but very little more worn than upon the tangents while upon the P. R. R. the wear of outer rails on curves has been much more rapid.

Generally speaking, it has been found to require less coal to run freight trains at an average speed of 18 to 20 miles an hour, than from 10 to 12 miles. But few time tables are arranged in accordance with the gradients of the road. On some roads uniform speed is required over all portions of the road, which practically lessens the number of cars drawn per train; while if the time tables were arranged in accordance with the work, from two to three more cars could be taken on the ordinary trains. The construction of cars for the same purpose is so different in detail, that their resistance to traction varies; so that it seems impossible to determine anything more than an approximate formula for general application. Changes are made upon mere opinion, without a knowledge of facts. In locomotives where perhaps more pains are taken to systematize matters, we find in those said to be made from the same drawings, a variation from ten to twenty per cent. in their capacity. This is a common observation of engineers and master mechanics, derived from their daily experience. Some engines will give a very smooth diagram, while others will show great irregularities (on the same track), due to steam admission or counter balance. Each engineer gives a personal equation to the diagram. The rate of adhesion varies also for the same weight in different engines, and is much greater at slow speeds than at high ones with the same engine.

In drawing freight trains the greatest range of variation in resistance is due to the wind; stock cars giving the highest rate. Loaded box cars in trains of twenty to twenty-five cars give on a level and straight track from four to six lbs. resistance per ton; while thirty empty stock cars gave 13.20 lbs. per ton on a windy day. Trains are now limited in length, from the uncertainties of changes of weather during their transit. As soon as the matter is more thoroughly understood by the railroad people, I hope to see trains dispatched in accordance to the Signal Service indi-

cations. The value to railroad transportation of such knowledge can hardly be estimated.

Nearly all the experiments are conducted by private individual enterprise, and few of the important problems of transportation have been touched. There is so much jealousy that but few care to know anything about the problems of transportation, as it more or less affects the opinions of the managers.

From many numerous experiments we have just completed for one railroad, we have determined the cost of moving a ton of weight one mile, and plotted the results, showing on some of its rates it did not get back the cost of simply moving its cars, engines, and freight, regardless of any interest and cost of organization.

There are some attachments designed and yet to be added to the Dynagraph for special experiments.

Mr. ABBE followed with further explanations, and some points were discussed by Messrs. HARKNESS and DUDLEY.

Mr. C. A. WHITE read a communication on

THE FRESH-WATER SHELL-HEAPS OF THE INTERIOR RIVERS OF NORTH AMERICA.

Messrs. BAIRD, MASON, ABBE, and GILL added information respecting shell-heaps in other regions, especially near the coasts of Massachusetts, Maine, and New Brunswick, and discussing the evidence of cannibalism found in some of them.

158TH MEETING. MARCH 15, 1879.

Vice-President HILGARD, and subsequently the President, in the Chair.

Fifty-eight members and visitors present.

Prof. A. WINCHELL, of Syracuse, N. Y., made a communication on

THE PROGRESSIVE DISPERSION OF MANKIND OVER THE SURFACE OF THE EARTH,

referring mainly to prehistoric races.

Remarks were made by Messrs. HILGARD, POWELL, DALL, ALVORD, PARKER, MASON, WARD, ABBE, WHITE, GILL, WOODWARD, HARKNESS, and WINCHELL, the discussion taking a wide range.

Mr. J. R. EASTMAN exhibited and described

A PERSONAL EQUATION INSTRUMENT

devised by him in 1875, and successfully used at the Washington Observatory in determining the absolute as well as relative equations of observers with the transit instrument.

Remarks were made by Mr. PAUL.

Mr. J. E. HILGARD exhibited two

PHOSPHORESCENT CLOCKS,

stating that the faces were coated with calcium-sulphide, and that after exposure to solar light they would remain luminous in the dark during the greater part of a night.

Mr. ANTISELL explained that a number of sulphides possess this property of phosphorescence.

159TH MEETING. MARCH 29, 1879.

The President in the Chair

Thirty-eight members and visitors present.

Mr. S. NEWCOMB made a communication on

THE RECURRENCE OF ECLIPSES.

The subject was discussed by Messrs. ABBE and TAYLOR.

Mr. L. F. WARD read a paper on

THE ORIGIN OF THE CHEMICAL ELEMENTS.

Dr. W. K. BROOKS, of the Johns Hopkins University, read a paper on

THE EMBRYOLOGY OF LIOGULA AND THE SYSTEMATIC RELATIONS OF THE BRACHIOPODS,

and illustrated the comparative characteristics of the Brachiopods in their embryonic condition with those of the Polyzoans.

160TH MEETING. APRIL 12, 1879.

The President in the Chair.

Fifty-one members and visitors present.

Mr. G. K. GILBERT made a communication

ON THE KANAB BASE-LINE, AND A PROPOSED NEW METHOD OF BASE MEASUREMENT.

(ABSTRACT.)

1st. *The Measurement of the Kanab Base.*

The base lines of the Powell Survey have been measured with wooden rods. The base at Kanab, Utah, was measured in 1878 with apparatus prepared originally by Mr. A. H. Thompson. The measurement was performed under the direction of Mr. Gilbert by Mr. J. H. Renshawe. The apparatus consists essentially of two 15-foot rods applied to each other, end to end, in alternation. They are provided with suitable accessories to regulate their alignment and height, and to record their inclination. They were compared before and after use with standard steel rods furnished by the Coast Survey. Two measurements were made, and the second result was found to exceed the first by 0.84 of an inch.

It has been stated by Mr. Hayden that the results attained by Mr. Powell by the use of wooden rods are less accurate than those given by the steel tape (45th Congress, 2d Sess., House Mis. Doc. No. 55, p. 25). It is desirable to test the truth of this statement, for if wooden rods do not give greater accuracy than steel tapes they should be discarded on the score of economy. A satisfactory comparison of the two methods is not yet possible for the reason that the results with the steel tape have not afforded data for the computation of probable error, but a first impression may be derived from the comparison of the discrepancies between duplicate measurements. The following table includes the only base twice measured by the Powell Survey, and the only bases published by the Hayden Survey. The last column shows the estimated probable discrepancy for a common distance of 4.3 miles.

Name of Base line.	Name of Survey.	Approximate length in miles.	Differences of two measurements in feet.	Comparative differences.
Kanab	Powell	4.3	.070	.07
San Luis*	Hayden	5.4	.18	.16
Denver*	Hayden	6.0	1.798	1.51

There seems nothing in these figures to require a discontinuance of the use of wooden rods.

The Kanab base was divided into 44 sub-equal sections, and two independent measurements were made of each section. From the 44 resulting discrepancies the probable error of the whole measurement was computed and found to be .09 ft. or 1-250,000 of the whole length of the base. When a similar test has been applied to the steel tape, the relative value of the two apparatus can be better judged.

Unfortunately the fraction 1-250,000 does not represent the total error of the base line as determined, but only that portion of the error which depends on the manipulation of the wooden rods; another portion introduced in the comparison of the rods with the steel standards is much greater. The latter affects the value of the Kanab base line, but is independent of the value of the apparatus.

2d. *A proposed New Apparatus and Method.*

In some methods of base-measurement a single unit of length is applied repeatedly. A record is made each time of the position of the advance end, to which record the rear end is afterward applied. This is the usual practice when a chain or tape is used. In other methods two unit measures are used in alternation. One remains stationary while the other is carried forward, and the rear end of the moving measure is applied to the advance end of the stationary. This is the usual practice when wooden or metallic rods are employed. It is now proposed to combine the method of the single unit and record with the use of the rod.

Where two rods have been used a simple contact of their ends has been found impracticable, and an observation has usually been substituted. Where a single unit has been used the record has been of the nature of an observation and subject to personal equation. It is proposed to replace the observations by an automatic record.

In the use of two rods it is necessary, for each added unit, to carry forward two tripods and adjust them in height. It is pro-

* U. S. Geol. and Geog. Survey of Colorado, 1876, p. 280.

posed to carry forward only one tripod for each unit, and to give it no vertical adjustment.

To execute this plan a novel rod is proposed and a number of tripods of a novel pattern.

The Rod. The material is not essential. To the under surface at each end is attached a steel sphere one-half inch in diameter. The distance between the centres of the spheres is the unit of length. Some appliance must be attached for the reading of inclination.

The Tripods. The head of each is broad, and upon it rests a free plate bearing a conical socket to receive one of the spheres attached to the rod. A device is added to lift the plate upon balls at will, and another device to clamp it.

The Use. Suppose one of the tripods, with clamped plate, to stand so that the conical socket is in the line of the base. Another is placed in advance at a distance approximately equal to the rod length, and its plate is unclamped. One sphere of the rod is now placed in the fixed socket and the other in the movable. The advance end of the rod is aligned. The balls are lifted under the movable plate for an instant, to substitute rolling for sliding friction, and relieve all strains. The plate is clamped. The inclination of the rod is observed. The advance socket has now become a record of the application of the unit of length. A third tripod is placed on the line and the rod is carried forward to repeat the process.

The apparatus is proposed with special reference to the needs of such work as that of the Powell Survey. It is hoped that it will combine a high degree of precision with noteworthy rapidity of manipulation.

The subject was discussed by Messrs. SCHOTT, ABBE, JENKINS, POWELL, and DOOLITTLE.

Mr. W. H. DALL communicated the results of his observations on

THE MUSCLES OF THE OYSTER,

and called attention to the existence of a small anterior muscle, which he considered to be a pedal one.

The subject was commented on by Messrs. WHITE and GILL.

Mr. C. E. DUTTON commenced a communication on

THE SUCCESSION OF VOLCANIC ERUPTIONS.

This communication being unfinished when the hour for

adjournment arrived, the completion of it was postponed to the next meeting.

The Society then adjourned.

161ST MEETING. APRIL 26. 1879.

The President in the Chair.

Forty-eight members and visitors present.

The minutes of the last meeting were read and adopted.

Mr. J. J. WOODWARD made a communication on

A NEW APERTOMETER FOR MICROSCOPIC OBJECTIVES.

Mr. DUTTON continued and completed his communication on the succession of Volcanic Eruptions, reserving his paper for ublication.

Remarks were made by Messrs. POWELL, ELLIOTT, ANTISELL, GILBERT, TAYLOR, FARQUHAR, and OSBORNE.

Mr. O. T. MASON made a communication illustrated by impressions of the plates of the lately published works of Dr. Habel on

THE DECIPHERMENTS OF SOME AZTEC MONUMENTS LATELY DISCOVERED IN GUATEMALA.

The meeting then adjourned.

162D MEETING. MAY 10, 1879.

The President in the Chair.

Thirty-eight members and visitors present.

The minutes of the last meeting were read and adopted.

Mr. J. R. EASTMAN read a communication on

A PERSONAL EQUATION APPARATUS.

This was in the nature of a personal explanation of a former communication by himself upon the same subject.

Remarks were made by Mr. HILGARD.

Mr. E. B. ELLIOTT gave a communication on

THE SUBJECT OF INTERNATIONAL COINAGE.

In explanation of the progress lately made towards an international coinage he referred to the action of the Japanese and Argentine governments in the adoption of coins of gold, weighing exactly five grammes, and to the measure moved by Mr. Garnier, in the French legislature and recommended by a committee of that body, to coin a gold piece weighing five grammes.

He then explained the so-called Warner bill, or the measure now pending in the House of Representatives, and showed how it differed in respect to the coinage of silver from laws now in force. He then showed by means of a diagram the fluctuations of the value of silver relative to that of gold since the year 1792.

Remarks were made by Mr. BURCHARD.

Mr. G. K. GILBERT made a communication on

AIR CURRENTS ON MOUNTAIN SLOPES.

He stated that in the mountains of the West the air currents at night usually blow down the slopes; and blow up the slopes during the day, when the general state of the atmosphere is calm. He referred to the discussion of this subject by Mr. Loew, who stated the opposite condition of facts. Mr. Gilbert gave the results of thirty observations, all of which conformed to the movement he asserted.

Remarks were made by Messrs. ABBE, ANTISELL, POWELL, and NEWCOMB.

The meeting then adjourned.

163D MEETING. MAY 24, 1879.

The President in the Chair.

Fifty-three members present.

The minutes of the last meeting were read and adopted.

Dr. J. R. M. IRBY, of the Johns Hopkins University, upon invitation of the General Committee of the Society, read a communication entitled

SOME OBSERVATIONS ON THE CRYSTALLINE STATE OF MATTER.

He first discussed the nature of crystals and their distinctions from the other states of matter, and remarked that the crystal is the most perfect expression of the molecular forces of matter with which we are acquainted, since it is the state of matter in which the equilibrium of those forces is least disturbed by heat motion.

In the second part of the paper Haüy's theory of the constitution of crystals was discussed, its importance insisted upon, and its reconsideration in connection with all the phenomena of crystals was urged.

In the third part of the paper the theory of Haüy was applied to the mineral species *calcite*. In the case of this mineral—one of the most Protean in its crystalline forms—the author thought the results so fully in accordance with the theory that much was to be hoped from its careful application to other crystals.

Dr. IRBY also adverted to the experiment of Harting, who produced forms similar to those of the coccoliths obtained from the ocean depths by treating the albumen of eggs with solutions of lime.

Remarks upon the paper were made by Messrs. ANTISELL and NEWCOMB.

The second communication was by Mr. HARKNESS, upon

THE COLOR CORRECTIONS OF ACHROMATIC OBJECTIVES.

(ABSTRACT.)

1st. From any three pieces of glass suitable for making a corrected objective, but not fulfilling the conditions necessary for the complete destruction of the secondary spectrum, it will always be possible to select two pieces from which a double objective can be made that will be superior to any triple objective made from all three of the pieces.

2d. The color correction of an objective is completely defined by stating the wave-length of the light for which it gives the minimum focal distance.

3d. An objective is properly corrected for any given purpose when its minimum focal distance corresponds to rays of the wave-length which is most efficient for that purpose. For example: in an objective corrected for visual purposes the rays which seem brightest to the human eye should have the minimum focal distance; while in an objective intended for photographic work the rays which produce the greatest effect upon silver bromo-iodide should have the minimum focal distance.

4th. In the case of a double achromatic objective, the secondary spectrum (or in other words, the diameter, at its intersection with the focal plane, of the cone of rays having the maximum focal length) is absolutely independent both of the focal length of the combination, and of the curves of its lenses; and depends solely upon the aperture of the combination, and the physical properties of the materials composing it.

5th. When the focal curve of an objective is known; and the relative intensity, for the purpose for which the objective is corrected, of light of every wave-length, is also known; then the exact position which the focal plane should occupy can be readily calculated.

Incidentally, it may be remarked that in an objective corrected for photographic purposes the interval between the maximum and minimum focal distance is less than in one corrected for visual purposes. Hence a photographic objective has less secondary spectrum, and is better adapted for spectroscopic work, than a visual objective.

Prof. A. HALL read a paper entitled

NOTES ON THE ORBITS OF TITAN AND HYPERION.

He stated that during the past winter he had collated and reduced all observations of Hyperion (the seventh and faint satellite of Saturn) that have been made since its discovery in 1848 by the Bonds at Cambridge, and by Lassel in England. The observations made in 1848 by the Bonds were not well adapted to the determination of its orbit, since the plane of the orbit was seen edgewise. In 1852 the plane of the orbit having opened out, Lassell made a good series of thirty observations, from which Prof. Hall computed a set of elements that fix the position of the satellite in its orbit with a good degree of certainty for that epoch. In the year 1875 a series of observations was begun with the Washington 26-inch refractor and continued to the present time, and comparing the elements deduced from them for the present epoch with those of 1853 it is possible to determine the periodic time of the satellite and the motion of its apsides. In order to

determine small inequalities in the periodic time and motion of apsides, it will be necessary to wait until the orbit is opened out sufficiently to observe the satellite completely around the planet. Another result of the observations is that Hyperion has a larger radius vector or is moving in a larger orbit than that which is due to its periodic time and Bessel's mass of Saturn. This he thinks arises from the action of the large satellite Titan, whose orbit is very near that of Hyperion, and the two satellites sometimes approach each other very closely.

Prof. HALL remarked upon the great complexity of the Saturnian system, and the relatively great perturbations to which it is subject from the extreme oblateness of the planet, from the similar effects of its ring, from the attractions of the satellites upon each other, and from the attractions of Jupiter and the Sun, thus rendering it a most interesting and instructive subject of contemplation and study.

164TH MEETING. JUNE 6, 1879.

The President in the Chair.

Fifty members present.

The minutes of the last meeting were read and adopted.

The proceedings for the evening consisted in the communications of Messrs. C. V. RILEY and SIMON NEWCOMB.

The first paper by Mr. RILEY was entitled—

PUPATION OF THE NYMPHALIDÆ.

(ABSTRACT.)

There is no more interesting phenomenon in insect transformation than the withdrawal of the chrysalis from the shrunken larval skin and its firm attachment to the button of silk previously spun by the larva, in those *Rhopalocera* which suspend themselves perpendicularly during pupation. For a century and a half Reaumur's account, namely, that the soft segments of the forming chrysalis acted the part of legs by grasping the larval skin between the sutures, has been accepted and generally copied. Dr. J. A. Osborne, of Milford, England, first drew attention, two years ago (in *Nature*, vol. xvi. pp. 502–3), to the fact that there was a membrane concerned in the act, and Mr.

W. H. Edwards, of Coalburgh, West Virginia, corroborated Dr. Osborne's statement by observations on some of our American species, recorded in the *Canadian Entomologist* of last December. Prof. Riley records the result of a number of observations on this subject, and thus explains the philosophy of the act which has so generally misled observers. His studies have been made principally with the larvæ of *Vanessa antiopa.* The principal means by which the chrysalis holds on and rises at the critical moment, is a stout ligament, which is, virtually, the shed intestinal canal; not alone the lining, but the whole organ, which, as we know, becomes sub-obsolete in the imago state of so many *Lepidoptera.* It is the ilium and colon, more particularly, which are serviceable, and the ligament holds with such force around the anus of the cast larval skin that it cannot easily be severed. The rectum of the nascent chrysalis draws this in, or lets it go, by peristaltic action of the sphincter muscles, the whole ligament being drawn out as soon as the hooks of the cremaster reach the silk. In addition to this ligament, which is of a reddish color, there are two lateral ligaments, also quite long and strong, and of the color of the skin, which serve as auxiliaries. These are the shed linings of the tracheæ issuing from the last or ninth pair of spiracles, which in the chrysalis become closed or blind. These ligaments may be called the tracheal ligaments, and seem to be somewhat specialized to aid in this important act. Lastly, there is the membrane proper, referred to by Dr. Osborne, which is virtually but the anal portion of the skin itself, or corium, caught upon the knobs at the end of the ridges which usually form the ventral part of the cremaster. It consists chiefly of the skin that lines the region of the rectum and the anal prolegs, and takes on a more or less bifurcated form from the pulling power of the knobs during the act of withdrawal from the larval skin. These ligaments Prof. Riley considers constant physiological factors in the problem, most necessary in those species which have the knobs imperfectly developed, and acting even during the larval molts, and so holding the shed skin of *Lepidopterous* larva that it is worked to the anus in a shrivelled mass, as a stocking is pushed to the toes; whereas, in most other insects, and especially in those where the metamorphosis is incomplete and the change in the intestinal canal but slight, the exuviæ are crawled out of rather than worked off, the anal parts not being held within the end of the molted skin, but really being the first parts detached. The membrane is a purely mechanical factor, and may not always be properly caught and drawn out. It may also be severed without necessarily causing the chrysalis to drop. Yet that it is an important aid to the rising of the chrysalis there cannot be much doubt, and we find, in the chrysalis of *Paphia glycerium* for instance, a totally different mechanical provision for clutching the membrane, namely, a notch between the ridges around the rectum and the base of

the cremaster proper, in which the membrane may be caught; the ridges being, in this species, very narrow, smooth, and shallow, and the ordinary ventral knobs obsolete.

Mr. NEWCOMB's communication was entitled

A THERMODYNAMIC THEORY OF THE SPECTRUM.

The subject matter and its discussion were reserved by Mr. NEWCOMB for publication.

165TH MEETING. JUNE 21, 1879.

The President in the Chair.

Thirty-five members present.

The minutes of the last meeting were read and adopted.

The first paper was by Mr. J. R. EASTMAN, entitled

SOME RESULTS FROM THE DISCUSSION OF THE OBSERVATIONS OF THE TRANSIT OF MERCURY OF MAY 6, 1878.

(ABSTRACT.)

In response to a circular issued from the Naval Observatory, more than one hundred observations were made and forwarded to the Observatory, where they have been reduced and discussed, and will soon be printed.

Fifty-two observations were made of the first contact, eighty-three of second, eighty-two of third, and eighty of fourth.

In obtaining the final results each observation was assigned its appropriate weight according to the scale in which three represented the best observation.

Three observations of first, ten of second, ten of third, and seven of the fourth contact were given the weight three.

The results from the different weights for geometric observation are

	1st contact.	2d contact.	3d contact.	4th contact
	h. m. s.	h. m. s.	h. m. s.	h. m. s.
Weight 3	22 4 42.0	22 7 42.1	5 35 27.8	5 38 25.7
" 1 and 2 . .	50.7	39.5	28.3	17.9
" 1, 2, and 3 .	48.4	40.2	28.2	19.8

Assuming that the results from the observations with weight 3 best represent the true phenomena, the difference between the computed places from the data in the American and English ephemerides and the observed places are C.—O.

Contacts.	Amer. Eph. s.	Eng Eph. s.
I.	+ 77	— 29
II.	+ 84	— 22
III.	+ 110	+ 56
IV.	+ 119	+ 65

The attempt by many observers to determine the true time of contact, by noting the time of similar phases before and after the true phase, generally failed, as did also the attempt to observe geometrical contact.

The method which was successful, and which is recommended in all similar observations of interior contact, was to observe for second contact the time when the first flush of light appeared between the limbs of the sun and planet, or the moment of the rupture of the "black drop." For third contact the breaking of the thin line of light between the limbs of the sun and planet or the formation of the "black drop" was taken as the true time of the phenomenon.

A thorough investigation of the motion of Mercury is greatly needed, and until this is done no reliable interpretation of these results can be made.

The differences between the computed and observed places of Mercury have been ascribed to the action of an intra-mercurial planet to meteor streams, and to the solar corona. Meteor streams have never been seen in such a position as to produce any such action.

In order to determine the action due to the corona it would be necessary to know its form, but as spectroscopic analysis, photographs, tracings of the images in the focus of the telescope, and naked eye sketches all give widely different limits to its extension, this solution is out of the question.

Mr. C. V. Riley then made a commnication entitled

THE ISSUANCE OF SILKWORM MOTHS FROM THEIR COCOONS, AND SOME STRIKING DEPARTURES FROM NORMAL HABITS IN INSECTS.

The final communication was from Mr. J. W. Osborne on

A CASE OF PECULIAR CORROSIVE ACTION ON METALLIC TIN.

The President then announced that, in conformity with a resolution by the general committee, the Society stood adjourned until the second Saturday in October.

166TH MEETING. OCTOBER 11, 1879.

The President in the Chair

Thirty-eight members and visitors present.

Mr. S. NEWCOMB communicated remarks on

A RECENT VISIT TO CALIFORNIA TO INSPECT A SITE FOR THE NEW LICK OBSERVATORY.

A discussion followed, which was participated in by Messrs. ALVORD, HOLDEN, and GALE.

Mr. W. H. DALL made a communication on

THE DEEP SEA DREDGINGS IN THE GULF OF MEXICO AND THE WEST INDIES IN 1873–1878, BY PROFESSORS LOUIS AND ALEXANDER AGASSIZ AND THE OFFICERS OF THE U. S. COAST SURVEY.

Remarks were made by Messrs. GILL, TAYLOR, and WARD.

Mr. E. B. ELLIOTT made a communication on

LARGE AREA ILLUMINATION BY ELECTRICITY.

The meeting then adjourned.

167TH MEETING. OCTOBER 25, 1879.

The President in the Chair.

Thirty-seven members and visitors present.

The minutes of the last meeting were read and adopted.

The President announced to the Society the election of WILLIAM FRANCIS RITTER and Lieut. FREDERICK COLLINS, U. S. Navy, as members of the Society; also the resignation of Commander GEORGE DEWEY, U. S. Navy.

Mr. C. A. SCHOTT made a communication on

THE SECULAR CHANGE IN THE MAGNETIC DECLINATION IN THE UNITED STATES AND AT SOME FOREIGN STATIONS.

(ABSTRACT.)

In this paper it is proposed to give a brief account of the present state of our knowledge respecting the secular change in the direction of the magnetic needle, as observed within the limits of the United States and at some adjacent stations—from the earliest to the present time.

The collection of the material and its discussion formed part of the work of the U. S. Coast and Geodetic Survey; the results have just been published in pamphlet form by the survey office. This paper contains in fifty quarto pages, first, an explanation of the secular motion, as compared with other motions to which the direction of the needle is subject; second, an exposition of the mathematical treatment for the representation of that motion; third, an extensive collection of results of about 525 observations at 52 stations; fourth, tables of the results of the discussion, comparison between observations and computations, and concludes with a table of decennial values of the magnetic declination from the earliest time of record to the present time. It is illustrated by a diagram and a chart, the former exhibiting the nature of the curve which conforms to the secular change, the latter illustrative of the positions of the line of no-declination at two epochs and of the region where the needle appears to be at present almost stationary; the annual change for 1880 is marked on it in figures.

The magnetic declination (commonly called the variation of the compass) varies with respect to space and time. It is a matter of observation that a magnet, when light and delicately suspended, is seldom or never at rest, but is always shifting its direction, or in a state of oscillation or of tremor, and may be in a state of sudden changes. These angular motions have been classified as regular or periodic, and as irregular variations; it is the first and largest of the periodic motions which claim our special attention. To distinguish it from other regular oscillations, a few explanatory remarks touching the principal laws of changes will suffice. The *solar-diurnal* variation consists in a systematic movement of the magnet having for its period the solar day. Its character is the same for the greater part of the northern hemisphere, viz.: about the time of sunrise the *north* end of the needle is generally found approaching to or near its most easterly deflection from the average magnetic meridian; this extreme position to the right is reached about 8 A. M., the north end then begins to move to the westward and reaches its opposite extreme position about half-past 1 P. M.; after this epoch the needle gradually returns to the morning position, undergoing more or less minor fluctuations. The range of motion is greater in summer than in winter; it is greater in the higher magnetic latitudes when the horizontal magnetic intensity is less than in lower latitudes; it is also subject to an eleven year inequality coinciding with the cycle of the sun spots—the greater

the spotted surface of the sun the greater the daily range of the motion of the needle and the less the activity of the sun in producing sun spots the less this daily magnetic motion. The angular range between the eastern (morning) and western (afternoon) elongations is, for instance, at Philadelphia about 8′ on the average of the year, at Key West, Florida, it is about 5½′, during August it is 10½′ at Philadelphia, and during November but 6′ at the same place, and is nearly double in amount during the maximum of sun spots as compared with the amount during the minimum period.

The *annual* variation is a small periodic change in the declination of at most 1½′ of arc.

The *lunar inequalities* are still smaller in extent, twice each lunar day or during 25 solar hours the magnetic needle is found subject to two oscillations, that is, there are two maxima and two minima with a range between them of about 27″ at Philadelphia and 38″ at Toronto, Can. These may be compared with the moon's tidal action producing two high and two low waters each lunar day, and the magnetic effect may possibly be due to change in the lunar gravitation which brings the terrestrial spheroid twice each day into a state of constraint and release alternately. Possibly this curious effect as well as the solar inequality may ultimately depend on changes of heat, which is known to affect the intensity of magnetism.

Magnetic disturbances or storms may occur at any time, though they cannot be predicted, yet when treated by the established method they are found subject to various laws. They consist of sudden and sometimes of great deflections or of irregular wavy motion and may continue for a day or even for several days; they are frequently accompanied by auroral lights and by strong electric earth currents. They likewise depend on the condition of the sun with respect to spots.

The *secular change* of the declination is supposed to be of periodic character, requiring centuries for its full development; the motion, may be compared with that of an oscillating pendulum which comes to rest momentarily at the extreme positions or elongations and moves fastest midway between. Smaller variations within the great period have been detected in the direction of the needle. About the time of the maximum deflection the magnet appears almost stationary for several years, but soon a progressive motion commences, and, at first increasing, afterwards diminishing its rate until the opposite stationary position is reached and the motion reversed. Possibly this kind of a "swing" may be repeated. Observation indicates that a *complete* oscillation requires between 2½ and 3½ centuries, during which time the magnet would swing twice through several degrees. Thus, at New York city the direction of the needle was observed to be nearly invariable about 1685, pointing then nearly 9° to the west of north, it then moved easterly and reached its

easternmost digression about 1797, showing at the time only 4° west declination. Ever since this epoch the motion of the north end has been westerly, its present value being nearly 7¾° west. The greatest annual change, 5′ nearly, has apparently been passed. These stationary epochs are different for different localities, the last one was noted earliest in Maine; later in Florida and Texas, and it has not yet been attained in California, where easterly declination is still slowly on the increase. Thus, the easterly stationary condition was reached at Portland in 1764, at Boston in 1777, at Washington 1796, at Savannah 1809, at New Orleans 1831, and at San Blas in Mexico in 1849. Excepting a certain region along our Pacific Coast, as indicated on the chart above referred to, the effect of the secular change at present is to *increase* the west declinations, or, what is the same thing, to *diminish* the east declinations. The same seems to take place in Alaska.

This secular change is conveniently expressed by a circular or harmonic function, viz:—

$$D = \delta + r \sin(\alpha\, m + c) + r_1 \sin(\alpha_1\, m + c_1) + \ldots.$$

when D = the magnetic declination at any time t
m = the number of years (and fraction) from an adopted epoch $t_0 = 1850$, hence $m = t - 1850{\cdot}0$.
$\alpha\ \alpha_1 \ldots.$ are factors depending on the adopted periods $\rho\ \rho_1 \ldots.$
so that $\alpha = \dfrac{360°}{\rho}$ $\alpha_1 = \dfrac{360°}{\rho_1}$ etc.
$r\ r_1 \ldots$ are parameters and $c\ c_1 \ldots$ epochal constants of the several periodic terms.
δ = a constant representing the mean or normal direction of the needle about which the secular motion takes place.

Thus, for each place for which we have a sufficient number of observed declinations we have to determine four unknown quantities, viz., δ, r, α and c for the establishment of the first or principal term and three for each following term. This is done by application of the method of least squares, each observation furnishing a conditional equation of the form

$$o = \delta_1 - D + x + \sin \alpha\, m.\ y + \cos \alpha\, m\ z + \ldots.$$

supposing α has been suitably assumed and where $\delta = \delta_1 + x$
$y = r \cos c$ and $z = r \sin c$. The process must be repeated for a value $\alpha + d\,\alpha$ and so on until the sum of the squares of the differences of the observed and computed values equals a minimum. The second periodic term may best be established by Cauchy's method of interpolation.

The annual change v is found by

$$v = 60 \sin 1° \left[r\, \alpha \cos(\alpha\, m + c) + r_1\, \alpha_1 \cos(\alpha_1\, m + c_1) + \ldots\right]$$

expressed in minutes, and maxima and minima are found by putting the expression within the brackets equal to zero, from which equation m can be found. The apparent probable error of an

observation is deduced from the differences $\triangle$ of the n observed and computed declinations, and is expressed by $e_0 = \sqrt{\frac{0.455\ \Sigma\Delta_2}{n - n_1}}$ where n = the number of unknown quantities in the equation which were found from the observations themselves. The principal uncertainty in the investigation arises from comparatively large observing errors in the older observations and from the fact that the observations are made at different places in the same general locality, thus introducing possibly local deflections. For Philadelphia the deflecting force, when greatest, is estimated at about $\frac{1}{17}$ of the horizontal force.

To illustrate the above formula we have the expression for the secular change for New York

$D = +6^\circ.43 + 2^\circ.29 \sin(1.6\ m - 5^\circ.5) + 0^\circ.14 \sin(6.3\ m + 64^\circ)$ with the following table of observed and computed values, where + indicates *west* deflection.

NEW YORK. Latitude 40°42′.7. Longitude 74°00′.0 W. of G.

Year of Observation.	Observed Declination.	Computed Declination.	O—C or Δ
1684.5	+8°.75	+8°.80	—°.05
1691.5	8 .75	8 .68	+ .07
1724.0	7 .33	7 .50	— .17
1750.5	6 .37	5 .85	+ .52
1755.5	5 .00	5 .46	— .46
1789.5	4 .33	4 .30	+ .03
1824.5	4 .67	4 .64	+ .03
1834.5	4 .83	5 .17	— .34
1837.5	5 .67	5 .37	+ .30
1840.6	5 .45	5 .61	— .16
1841.5	6 .10	5 .68	+ .42
1844.6	6 .22	5 .92	+ .30
1845.7	6 .42	6 .01	+ .41
1846.3	5 .56	6 .05	— .49
1847.8	5 .68	6 .16	— .48
1855.6	6 .72	6 .73	— .01
1860.7	6 .73	7 .03	— .30
1873.8	7 .62	7 .59	+ .03
1874.6	+7 .38	+7 .62	— .24

Number of observations 19; apparent probable error of an observation ± 15′; time of last stationary epoch, easterly digression, 1797; amount at easterly digression + 4°. 0; annual change (increase) in 1870 + 2′.4, and in 1880 + 2′.5.

For San Francisco, California, we have the expression $D = -13^\circ.34 + 3^\circ.23 \sin(1.00\ m - 130^\circ.3)$ and the corresponding values: number of observations 15, probable error of an observation ± 8′, time *expected* for next stationary epoch, easterly digres-

sion 1890; declination at that time —16°.6; annual change in 1870 —1′.0, and in 1880 —0′.5. At this place the earliest observation dates from 1792.9 and the latest 1879.2.

Results similar to the above are given for 52 stations; of these, several are foreign, viz.; Halifax, N. S.; Quebec, Can., and York Factory on the Hudson Bay; Havana, Cuba, Kingston, Jamaica, Rio Janeiro, Vera Cruz, Mex., Mexico City, Panama, New Granada and Acapulco, San Blas and Magdalena in Mex.; Kailua, and Honolulu, Sandwich Islands, and Petropavlovsk, Kamtchatka.

Respecting errors of observations it is estimated that the observations made by Hudson in 1609 in the vicinity of our coast and those of Champlain made about the same time may be subject to a probable error of ± 4°. Observations in the 17th century were frequently made on board ship in preference to terra firma, as the land was supposed to attract the needle. The observations made by Vancouver on the western coast between 1792 and 1794 are subject to a probable error of only ± 1°, and this is about the present limit of uncertainty of observation taken at sea with the azimuth compass and under favorable conditions, whereas, with our present portable declinometers the observing error is below 1′, requiring a station to be occupied several days in order to eliminate the daily regular and irregular fluctuations of the magnet from the final resulting direction.

The tables containing the decennial values of the magnetic declination, as derived from the formulæ, should not be extended beyond the limit given to them (1885), though the expressions may continue to represent the phenomenon, of the cause of which no satisfactory explanation has ever been offered; they may also at any time fail; in fact they need continued attention and adaptation for every new observation or development, and this must continue so long as the process remains a tentative one, and we are without an adequate theory to guide us.

Remarks upon Mr. Schott's paper were made by Messrs. Harkness, Alvord, and Elliott.

Mr. J. S. Billings, Vice-President of the National Board of Health, made a communication on

THE WORK OF THE NATIONAL BOARD OF HEALTH,

stating the various subjects to which its inquiries and investigations had been directed and the progress made.

Remarks were made by Messrs. Mason, Osborne, Antisell, Newcomb, Woodward, and Toner, and the discussion extending to the disinfection of ships.

Mr. F. M. GUNNELL, by request, gave an account of attempts in the navy to disinfect ships infected by yellow fever, naming several which had been sent to Portsmouth, N. H., and after the exposure to the severe cold of an entire winter, on going to a warm climate, were again visited by the same disease. He stated also one case where the crew and stores having been removed, the ship was thoroughly steamed, and no cases subsequently appeared on the return of the crew.

The meeting then adjourned.

168TH MEETING. NOVEMBER, 1879.

The President, Mr. SIMON NEWCOMB, in the Chair.

Forty-seven members present.

The minutes of the last annual meeting were read and adopted.

The order of proceedings for annual meetings adopted Nov. 2, 1872, by the General Committee, were then read for the information of the Society, together with a list of members elected since the last annual meeting, and a list of the members entitled to vote at the annual election of officers.

Letters from Messrs. J. H. C. COFFIN and ASAPH HALL, declining to accept office under the Society, were then read, and are now filed in the records of the General Committee.

The Society then proceeded to ballot informally for President for the ensuing year, Messrs. PAUL and FARQUHAR being appointed tellers. As a result of the ballot Mr. SIMON NEWCOMB received a majority of the votes cast, and the informal vote was declared to be formal and made unanimous.

The next ballot was for Vice-Presidents. Messrs. BARNES, TAYLOR, HILGARD, and WELLING, having each received a majority, were elected, the informal vote being made formal.

In the choice of a Treasurer Mr. CLEVELAND ABBE was elected unanimously.

The election of two Secretaries then followed, and Mr. THEODORE GILL having received a majority vote was elected at the first ballot. At a second ballot Mr. E. S. HOLDEN was elected.

The nine members of the General Committee were then bal-

loted for. Upon an informal ballot it appeared that Messrs. Dutton, Elliott, Harkness, Powell, Schott, Toner, and Woodward had a majority, and on motion they were declared elected. Upon a second ballot Mr. Garrick Mallery was elected, and upon the sixth ballot Mr. W. H. Dall was elected.

It was then moved by Mr. Hilgard, and carried, that when this meeting adjourns it adjourn for two weeks, and that the adjourned meeting be regarded as a continuation of the annual meeting for the purpose of receiving the annual address of the President of the Society.

The Society then adjourned.

169th Meeting. November 22, 1879.

The President in the Chair.

Fifty-two members present.

The minutes of the preceding meeting were read and adopted.

The order of exercises for the evening, pursuant to the terms of the adjournment of the preceding meeting, consisted in the delivery of the annual Address of the President of the Society.

Mr. Simon Newcomb, the newly elected President, arose and stated to the Society that the pressure of other duties had prevented him from preparing an address upon the subject originally contemplated by him for this occasion.

He regretted this inasmuch as it seemed to him that the subject was better adapted to the spirit and purpose of an annual address to a Philosophical Society than the one which he had finally adopted. The paper chosen for the evening had been originally prepared with another object in view, but seemed to him not wholly unadapted to the occasion.

The President then read for his address a paper entitled

THE FUTURE OF THE HUMAN RACE REGARDED FROM THE STANDPOINT OF EVOLUTION,

which was listened to with great interest and pleasure by the Society. The paper was reserved by the President for revision.

At the conclusion of the reading the Society adjourned at 9.15 P. M. for the purpose of conversation and social intercourse.

170TH MEETING. DECEMBER 6, 1879.

Vice-President WILLIAM B. TAYLOR in the Chair.

Twenty-four members present.

The minutes of the last meeting were read and adopted.

The Chair stated to the Society that Mr. E. S. HOLDEN, who had been elected at the meeting of November 8th to fill one of the Secretaryships of the Society, had been compelled to decline the position on account of the requirements of his professional duties, and called upon the Secretary, THEODORE GILL, to read Mr. Holden's letter of declination. This letter will be found upon the records of the proceedings of the General Committee of the Society.

The Chair then announced that, conformably to the provisions of the Constitution of the Society, the General Committee had elected Mr. C. E. DUTTON to fill the position of Secretary in the place of Mr. Holden, declined.

The order of exercises for the evening was announced.

1. Mr. J. J. WOODWARD—Some Apparatus recently brought into use by the Medical Department of the Army for the Examination of the Eye.

2. Mr. MARCUS BAKER—Discussion of a Geometric Problem, with several solutions.

Dr. WOODWARD then explained to the Society the origin of the rules recently introduced into the Medical Department of the Army for the examination and testing of the powers of vision in recruits. The object of these rules was to obtain sufficiently accurate tests of those powers which enable the examining surgeon to discriminate between defects which would render a soldier unfit for the requirements of the military service and those which were insufficiently serious to cause his rejection.

The apparatus employed consisted first of a pack of test cards with circular spots on them four-tenths of an inch in diameter

and grouped in the ordinary manner of spots upon playing cards. The recruit would be required to distinguish readily the number of spots upon each card at the distance of twenty yards. By rule of simple proportion this would be equivalent to the recognition of a target three feet square at the distance of six hundred yards. The second part of the apparatus consisted of an instrument resembling an optometer, having a graduated beam twelve inches in length, carrying a slide capable of being clamped at any part of the length. In the holder is the mounting for lenses, two of which are provided, one of ten inches the other of four inches solar focus. For measuring myopia and hypermetropia a small card bearing a printed sentence in small type is placed in the slide which is first clamped at ten inches from the lens. At this distance the normal eye should be able to read the printed sentence through the 10″ lens, and have its accommodation relaxed. By moving the slide nearer to the eye the amount of myopia can be judged. By substituting the 4″ lens and moving the slide away from the eye beyond the four inch mark, the amount of hypermetropia can be judged. To ascertain the extent of astigmatism, there is substituted in place of the printed sentence a small card dial having two pairs of parallel lines crossing each other at right angles. The dial can be rotated in a vertical plane. Astigmatism, whenever it occurs, arises from the fact that the crystalline lens of the eye is not isotropic—different meridians having different curvatures. If the disc be placed at a distance from the eyes at which it would be focussed by the meridian of maximum curvature, and the dial turned so that one pair of lines is parallel to the plane of that meridian, only one pair of lines will be visible to the ordinary astigmatic eye. If the dial be rotated slowly this pair of lines will become indistinct and gradually disappear. The angle through which the dial is rotated before the disappearance will vary inversely with the degree of astigmatism, and thus the amount of rotation becomes a measure of the degree of astigmatism. This device also indicates the positions of the meridians of maximum and minimum curvature of the crystalline lens in the astigmatic eye.

For testing color-blindness, Dr. Woodward exhibited skeins of colored worsted of all the principal colors and of many tones. From a confused pile the recruits are required to sort out the colors into three groups—red, green, and violet. In case of

color-blindness, the errors committed will determine not only the existence of the defect, but also the particular colors which the eye is incapable of distinguishing.

DISCUSSION OF A GEOMETRICAL PROBLEM, WITH BIBLIOGRAPHICAL NOTES. BY MARCUS BAKER, U. S. COAST SURVEY, WASHINGTON, D. C.

The problem here discussed, and of which several solutions are given, is the following:—

In a right-angled triangle there are given the bisectors of the acute angles: required to determine the triangle.

This problem, like most problems in triangles in which the bisectors of the angles enter as a part of the data, cannot be solved by the elements of geometry, *i. e.* by the use of the circle and right line only. We shall give, first, trigonometrical solutions; second, algebraical solutions; third, constructions; and fourth, bibliographical notes.

FIRST SOLUTION.

Let α and β be the bisectors of the angles A and B respectively: then we have

$$AB \sin A = \beta \cos (45^\circ - \tfrac{1}{2}A) \text{ and } AB \cos A = \alpha \cos \tfrac{1}{2}A;$$

whence by dividing, remembering that

$$\frac{\cos (45^\circ - \frac{1}{2}A)}{\cos \frac{1}{2}A} = \frac{1 + \tan \frac{1}{2}A}{\sqrt{2}}.$$

$$\tan A = \frac{\beta}{\alpha\sqrt{2}}(1 + \tan \tfrac{1}{2}A) : \ldots \qquad (1)$$

and since

$$\tan A = \frac{2 \tan \frac{1}{2}A}{1 - \tan^2 \frac{1}{2}A},$$

we obtain by reduction

$$\tan^3 \tfrac{1}{2}A + \tan^2 \tfrac{1}{2}A + \left(\frac{\alpha}{\beta}\sqrt{8} - 1\right) \tan \tfrac{1}{2}A - 1 = 0 \ldots (2)$$

from which equation we may find $\tan \frac{1}{2}A$.

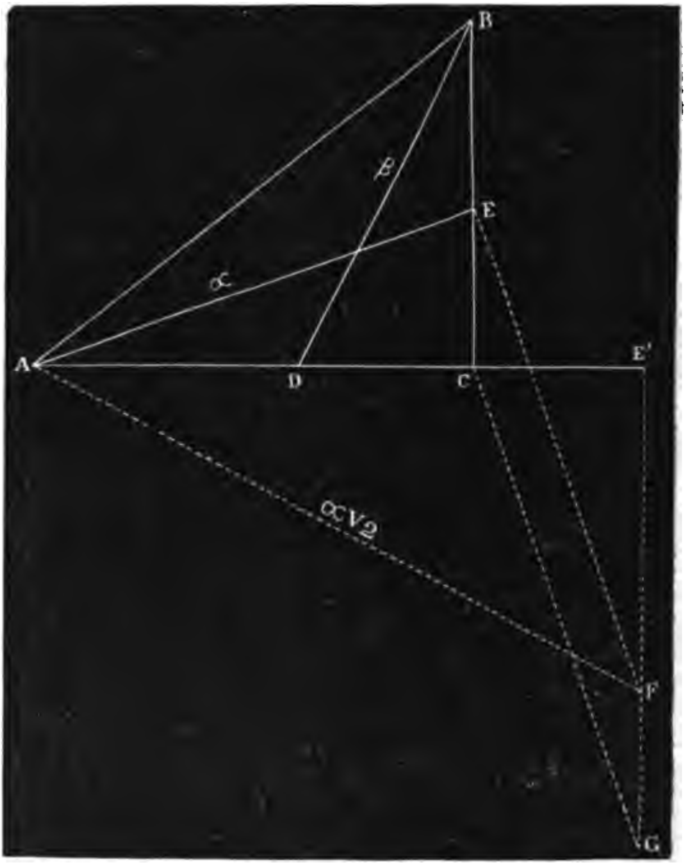
We may, however, obtain Eq. (1) directly from a construction as follows:—

Prolong A C to E′ making C E′ = C E, and from E′ draw E′ G perpendicular to A E′: from E draw E F perpendicular to A E, meeting E′ G in F; and from C draw C G parallel to E F. Now the triangle C E′ G is equal to the triangle A C E; hence

$CG = EA$, and also $EF = EA$: hence AEF is an isosceles right-angled triangle and $AF = a\sqrt{2}$. Also BDC and AFE' are similar triangles: whence

$$BC : AE' :: \beta : a\sqrt{2}.$$

Fig. 1.



Now when $AC =$ radius, or 1, $BC = \tan A$ and $AE' = 1 + \tan \frac{1}{2} A$: whence

$$\tan A = \frac{\beta}{a\sqrt{2}} (1 + \tan \tfrac{1}{2} A)$$

as before.

In this solution we have selected as our unknown quantity

In the above diagram, the symbol ∝ should be a, and ∝V2 should be $a\sqrt{2}$.

$\tan \frac{1}{2} A$. We might obviously have selected any other trigonometrical function, but this seems to lead to as simple a result as any.

If we make $\sin \frac{1}{2} A$ our unknown quantity our equation will be

$$\left\{ \begin{array}{c} 4\left\{(1-\frac{\alpha}{\beta}\sqrt{2})^2+1\right\}\sin^6\frac{1}{2}A-4\left\{\left(1-\frac{\alpha}{\beta}\sqrt{2}\right)\left(1-\frac{\alpha}{\beta}2\sqrt{2}\right)+2\right\} \\ \sin^4\frac{1}{2}A-4\left\{1+\frac{\alpha}{\beta}\sqrt{2}-\frac{2\alpha^2}{\beta^2}\right\}\sin^2\frac{1}{2}A-1=0, \end{array} \right\}$$

and if we make $\sec \frac{1}{2} A$ the unknown quantity our equation will be

$$\left\{ \begin{array}{l} \sec^6\frac{1}{2}A-2\left(3-\frac{\alpha}{\beta}\sqrt{8}\right)\sec^4\frac{1}{2}A+2\left(6-\frac{3\alpha}{\beta}\sqrt{8}+\right. \\ \left.\frac{4\alpha^2}{\beta^2}\right)\sec^2\frac{1}{2}A-4\left(1-\frac{\alpha}{\beta}\sqrt{8}+\frac{2\alpha^2}{\beta^2}\right)=0; \end{array} \right\}$$

whence it appears that the simplest equation is the one first obtained in which the tangent is made the unknown quantity.

Example.—Suppose $\alpha = 40$ and $\beta = 50$. Then our equation becomes

$$\tan^3\tfrac{1}{2}A+\tan^2\tfrac{1}{2}A+\left(\tfrac{4}{5}\sqrt{8}-1\right)\tan\tfrac{1}{2}A-1=0;$$

whence by Horner's method

$$\tan \tfrac{1}{2} A = 0.49788\ 15817\ 54736.$$

Whence

$$A = 37^\circ\ 03'\ 51''.33$$
$$B = 52^\circ\ 56'\ 08''.67,$$

and the sides of the triangle are

$$a = 35.807377$$
$$b = 47.407275$$
$$c = 59.41058.$$

SECOND SOLUTION.

Let a, b, and c be the sides of the triangle opposite A, B, and C respectively, and α and β as before; then we have (Fig. 1)

$$\frac{b}{\alpha} = \cos\tfrac{1}{2}A;\ \text{whence}\ \frac{2b^2}{\alpha^2} = 2\cos^2\tfrac{1}{2}A = 1+\cos A = 1+\frac{b}{c};$$

therefore

$$\frac{2b}{\alpha^2} = \frac{1}{b}+\frac{1}{c},$$

and similarly

$$\frac{2a}{\beta^2} = \frac{1}{a}+\frac{1}{c};$$

whence

$$\frac{2b}{a^2}-\frac{1}{b}=\frac{2a}{\beta^2}-\frac{1}{a}=\frac{1}{c}. \qquad (3)$$

Again

$$\frac{a}{\beta}=\cos\tfrac{1}{2}B=\cos(45^\circ-\tfrac{1}{2}A)=\frac{\sin\frac{1}{2}A+\cos\frac{1}{2}A}{\sqrt{2}};$$

whence

$$\sin\tfrac{1}{2}A=\frac{a\sqrt{2}}{\beta}-\cos\tfrac{1}{2}A=\frac{a\sqrt{2}}{\beta}-\frac{b}{a};$$

and since $\sin^2\frac{1}{2}A+\cos^2\frac{1}{2}A=1$,

$$\left(\frac{a\sqrt{2}}{\beta}-\frac{b}{a}\right)^2+\frac{b^2}{a^2}=1,$$

or

$$\frac{2a^2}{\beta^2}-\frac{2\sqrt{2}\,a\,b}{a\,\beta}+\frac{2\,b^2}{a^2}=1. \qquad (4)$$

If now we eliminate b between Eqs. (3) and (4) we have an equation from which a may be found.

From (4) we find, $b=\frac{a}{\beta\sqrt{2}}\left\{a\pm\sqrt{\beta^2-a^2}\right\}$ which substituted in (3) gives after some reduction

$$\frac{2\,a^2-\beta^2}{a}=\frac{\pm 2\,m\,a\sqrt{\beta^2-a^2}}{a\pm\sqrt{\beta^2-a^2}}$$

where $m=\frac{\beta}{a}\sqrt{2}$. This equation finally reduces to

$$(a^2-a\beta\sqrt{2}+\beta^2)\,a^6-(3\,a^2-3\sqrt{2}\,a\beta+2\,\beta^2)\frac{\beta^2}{2}\,a^4+(3\,a^2-2\sqrt{2}\,a\beta)\frac{\beta^4}{4}\,a^2-\frac{a^2\beta^6}{8}=0. \qquad (5)$$

THIRD SOLUTION.

Revolve the triangles B O E and D O A about B O and A O respectively so that E falls upon E′ and D upon D′, then

$$EOB=E'OB=E'OD'=D'OA=AOD=45^\circ,$$

and consequently B O D′ and A O E′ are right-angled triangles: hence

$$\frac{OE}{OA}=\tan\tfrac{1}{2}A,\ \text{or}\ \frac{a}{OA}=1+\tan\tfrac{1}{2}A;$$

whence $a=OA\,(1+\tan\frac{1}{2}A)$, (6)

and similarly $\beta=OB\,(1+\tan\frac{1}{2}B)$. (7)

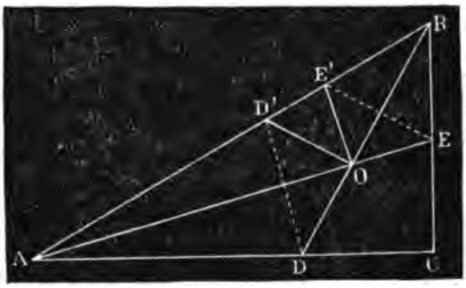
Again, $O A \sin \frac{1}{2} A = \gamma$; whence from (6)

$$\frac{\alpha}{\gamma} = \frac{1 + \tan \frac{1}{2} A}{\sin \frac{1}{2} A},$$

or $$\frac{\alpha}{\gamma} = \frac{1}{\sin \frac{1}{2} A} + \frac{1}{\cos \frac{1}{2} A},$$ and similarly

$$\frac{\beta}{\gamma} = \frac{1}{\sin \frac{1}{2} B} + \frac{1}{\cos \frac{1}{2} B}.$$

Fig. 2.



Now $\sin \frac{1}{2} B = \sin (45° - \frac{1}{2} A) = \frac{\cos \frac{1}{2} A - \sin \frac{1}{2} A}{\sqrt{2}}$ and

$$\cos \frac{1}{2} B = \cos (45° - \tfrac{1}{2} A) = \frac{\cos \frac{1}{2} A + \sin \frac{1}{2} A}{\sqrt{2}};$$

whence

$$\frac{\beta}{\gamma \sqrt{2}} = \frac{1}{\cos \frac{1}{2} A - \sin \frac{1}{2} A} + \frac{1}{\cos \frac{1}{2} A + \sin A} = \frac{2 \cos \frac{1}{2} A}{2 \cos^2 \frac{1}{2} A - 1},$$

from which we find

$$\cos \frac{1}{2} A = \frac{1}{\sqrt{2}} \left\{ \frac{\gamma}{\beta} \pm \sqrt{1 + \frac{\gamma^2}{\beta^2}} \right\},$$

and similarly

$$\cos \frac{1}{2} B = \frac{1}{\sqrt{2}} \left\{ \frac{\gamma}{\alpha} \pm \sqrt{1 + \frac{\gamma^2}{\alpha^2}} \right\}.$$

Since $\cos \frac{1}{2} B = \frac{\cos \frac{1}{2} A + \sin \frac{1}{2} A}{\sqrt{2}}$,

$$\sin \frac{1}{2} A = \left\{ \frac{\gamma}{\alpha} \pm \sqrt{1 + \frac{\gamma^2}{\alpha^2}} \right\} - \frac{1}{\sqrt{2}} \left\{ \frac{\gamma}{\beta} \pm \sqrt{1 + \frac{\gamma^2}{\beta^2}} \right\};$$

whence

$$\left\{ \frac{\gamma}{\alpha} \pm \sqrt{1 + \frac{\gamma^2}{\alpha^2}} \right\}^2 - \sqrt{2} \left\{ \frac{\gamma}{\alpha} \pm \sqrt{1 + \frac{\gamma^2}{\alpha^2}} \right\} \left\{ \frac{\gamma}{\alpha} \pm \sqrt{1 + \frac{\gamma^2}{\beta^2}} \right\}$$
$$+ \left\{ \frac{\gamma}{\alpha} \pm \sqrt{1 + \frac{\gamma^2}{\beta^2}} \right\} = 1. \qquad (8)$$

In this page, and the following, the symbol γ (for radius) should read r.

This equation involves only γ, the radius of the inscribed circle and the given bisectors of the angles α and β: hence we may determine γ from it. Eq. (8) becomes after a somewhat laborious reduction

$$64(\alpha^2-\alpha\beta\sqrt{2}+\beta^2)\gamma^6+8\sqrt{2}\alpha\beta(4\alpha^2-3\sqrt{2}\alpha\beta+4\beta^2)\gamma^4$$
$$+\alpha^2\beta^2(2\alpha^2-\alpha\beta\sqrt{2}+2\beta^2)\gamma^2-\alpha^4\beta^4=0. \qquad (9)$$

These three solutions just given all involve trigonometrical relations and are therefore properly classed as trigonometric solutions. They may all, however, be made independently of trigonometry. In the following we shall give the algebraical solutions corresponding to the first and second trigonometrical solutions together with a third and entirely independent solution.

ALGEBRAICAL SOLUTION.

From Fig. 2 we have

$$c:AD::a:DC::OB:OD::1:n$$
$$c:BE::b:EC::OA:OE::1:m;$$

from which

$$OB=\frac{\beta}{1+n},\ OD=\frac{\beta n}{1+n},\ OA=\frac{\alpha}{1+m},\ OE=\frac{\alpha m}{1+m},$$
$$AD=cn,\ CD=an,\ BE=cm,\ CE=bm.$$

Now

$$c.AD=OA^2+OB.OD \text{ or } c^2n=\left(\frac{\alpha}{1+m}\right)^2+n\left(\frac{\beta}{1+n}\right)^2,$$

$$c.BE=OB^2+OA.OE \text{ or } c^2m=\left(\frac{\beta}{1+n}\right)^2+m\left(\frac{\alpha}{1+m}\right)^2;$$

whence

$$m\left(\frac{\alpha}{1+m}\right)^2+mn\left(\frac{\beta}{1+n}\right)^2=n\left(\frac{\beta}{1+n}\right)^2+m\gamma\left(\frac{\alpha}{1+m}\right)^2,$$

or

$$\frac{m(1-n)}{(1+m)^2}\alpha^2=\frac{n(1-m)}{(1+n)^2}\beta^2. \qquad (10)$$

Again $b=AD+CD=n(c+a)\ \therefore\ a^2+n^2(c+a)^2=c^2$ and $a=CE+BE=bm+cm=mn(c+a)+cm$

$$=cm(1+n)+amn\ \therefore\ \frac{c}{a}=\frac{1-mn}{m(1+n)}.$$

Equating these two expressions

$$\frac{1-mn}{m}=\frac{1+n^2}{1-n}\ \therefore\ m=\frac{1-n}{1+n} \text{ and } n=\frac{1-m}{1+m};$$

substituting in (10) we find after reducing

$$\left.\begin{aligned} n^3 + n^2 + \left(\sqrt{8\frac{\beta}{\alpha}} - 1\right)n - 1 = 0 \\ m^3 + m^2 + \left(\sqrt{8\frac{\alpha}{\beta}} - 1\right)m - 1 = 0. \end{aligned}\right\} \qquad (11)$$

It is to be noted that $n = \frac{\mathrm{DC}}{a} = \tan\frac{1}{2}\mathrm{B}$ and $m = \frac{\mathrm{EC}}{b} = \tan\frac{1}{2}\mathrm{A}$, and therefore Eq. (11) corresponds to Eq. (2).

FIFTH SOLUTION.

The fundamental relations between the sides and bisectors are

$$\alpha^2 = \frac{bc\,(a+b+c)\,(-a+b+c)}{(b+c)^2} = (b^2 + 2\,bc + c^2 - a^2)\frac{bc}{(b+c)^2},$$

$$\beta^2 = \frac{ac\,(a+b+c)\,(a-b+c)}{(a+c)^2} = (a^2 + 2\,ac + c^2 - b^2)\frac{ac}{(a+c)^2}.$$

And since $a^2 + b^2 = c^2$

$$\alpha^2 = 2\,b^2\frac{c}{b+c} \text{ or } \frac{2\,b^2}{\alpha^2} = \frac{b+c}{c} = 1 + \frac{b}{c},$$

$$\beta^2 = 2\,a^2\frac{c}{a+c} \text{ or } \frac{2\,a^2}{\beta^2} = \frac{a+c}{c} = 1 + \frac{a}{c}.$$

Whence

$$\frac{2\,b}{\alpha^2} - \frac{1}{b} = \frac{2\,a}{\beta^2} - \frac{1}{a} = \frac{1}{c} \qquad (3)$$

as in the second solution, where this relation was obtained trigonometrically. Again

$$\frac{8\,a^2b^2}{\alpha^2\beta^2} = 2\left(1 + \frac{a}{c}\right)\left(1 + \frac{b}{c}\right) = 2\left\{1 + \frac{a+b}{c} + \frac{ab}{c^2}\right\} =$$

$$2\left\{\frac{a^2 + ab + b^2}{c^2} + \frac{a+b}{c}\right\} = \frac{a^2 + 2\,ab + b^2}{c^2} + 2\,\frac{a+b}{c} + 1$$

$$\therefore \frac{2\sqrt{2\,ab}}{\alpha\beta} = \frac{a+b}{c} + 1.$$

Again by adding

$$\frac{2\,a^2}{\beta^2} + \frac{2\,b^2}{\alpha^2} = \frac{a+b}{c} + 2.$$

Whence

$$\frac{2\,a^2}{\beta^2} - \frac{2\sqrt{2\,ab}}{\alpha\beta} + \frac{2b^2}{\alpha^2} = 1 \qquad (4)$$

as previously obtained trigonometrically. The solution is now completed as in the second solution.

SIXTH SOLUTION.

Let $OE = OE' = x$ (Fig. 2), $OD = OD' = y$, $AE = \alpha$, and $BD = \beta$; the angles marked with a dot are each equal to 45°, and therefore $EE' = x\sqrt{2}$, and $DD' = y\sqrt{2}$.

From similar triangles $BO : BD = OE' : DD'$, or $\beta - y : \beta :: x : y\sqrt{2}$. Whence

$$(\beta - y)\, y\sqrt{2} = \beta x. \tag{12}$$

$$(\alpha - x)\, x\sqrt{2} = \alpha y. \tag{13}$$

From (13)

$$y = \frac{\sqrt{2}}{\alpha} x\,(\alpha - x), \text{ and substituting in (12)}$$

$$\beta - \frac{\sqrt{2}}{\alpha} x\,(\alpha - x) = \frac{\alpha\beta}{2(\alpha - x)}.$$

Which reduces to

$$(\alpha - x)^2 x = \frac{\alpha\beta}{2\sqrt{2}}\,(\alpha - 2x).$$

Expanding, rearranging, etc., this reduces to

$$x^3 - 2\alpha x^2 + \alpha\left(\frac{\beta}{\sqrt{2}} + \alpha\right)x - \frac{\alpha\beta}{2\sqrt{2}} = 0 \tag{14}$$

CONSTRUCTIONS.

First Construction.—The equations obtained in the sixth solution point to a simple construction of the problem, as follows:—

Equations (12) and (13) may be written as follows:—

$$x^2 - \alpha x + \frac{\alpha}{\sqrt{2}} y = 0. \tag{15}$$

$$y^2 - \beta y + \frac{\beta}{\sqrt{2}} x = 0. \tag{16}$$

And each of these equations is the equation of a parabola. If these two parabolas be constructed, their intersection will determine x and y. The position and size of the parabola will readily appear by transforming co-ordinates. In equation (15) let

$$x = x' + \frac{\alpha}{2} \text{ and}$$

$$y = y' + \frac{\alpha}{2\sqrt{2}}, \text{ then}$$

$$x'^2 = -\frac{\alpha}{\sqrt{2}} y';$$

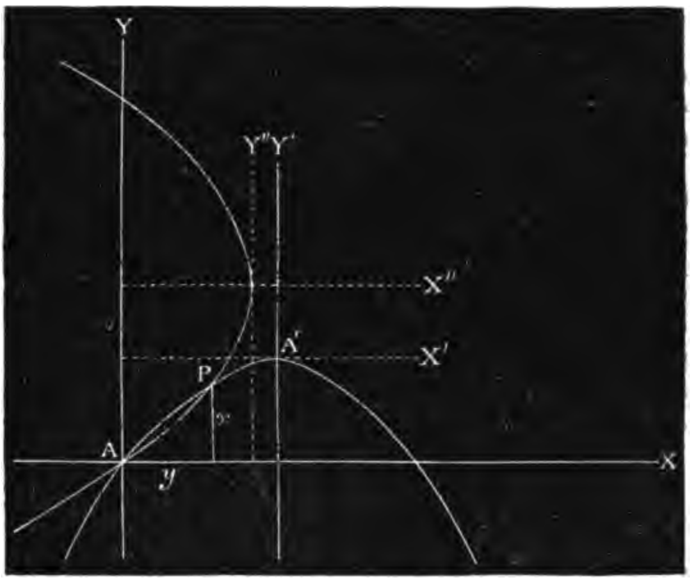
and in equation (16) let

$$y = y'' + \frac{\beta}{2} \text{ and } x = x'' + \frac{\beta}{2\sqrt{2}}, \text{ then}$$

$$y''^2 = -\frac{\beta}{\sqrt{2}} x''.$$

The following construction results immediately from the above. With reference to a set of co-ordinates X A Y construct a new set X′ A′ Y′ such that $x - x' = \frac{2}{\alpha}$ and $y - y' = \frac{\alpha}{2\sqrt{2}}$, and another set X″ A″ Y″ such that $x - x'' = \frac{\beta}{2\sqrt{2}}$ and $y - y'' = \frac{\beta}{2}$. With the first new set construct the parabola $x'^2 = -\frac{\alpha}{\sqrt{2}} y$, and with the second new set construct the parabola $y''^2 = -\frac{\beta}{\sqrt{2}} x''$;

Fig. 3.



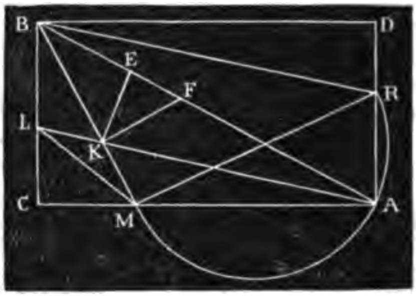
their intersections will determine the segments x and y, *i. e.*, O E and O D of Fig. 2. The construction is shown in Fig. 3.

Second Construction.—Take a rectangle A C B D, Fig. 4, and let A L, B M, the bisectors of A and B, intersect in K; then A K B = 135°. Through B draw B R parallel to A L to meet

A D in R; then $BR = AL$. Hence from data the triangle B M R is known.

It is well known that $2\triangle BMR + CM . DR = \text{rect. } AB = 2\triangle BMR + 2\triangle CLM$. (17)

Fig. 4.



Take $BE = BL$, $AF = AM$; then $\triangle BKE = \triangle BKL$, $\triangle AKF = \triangle AKM$, and $\triangle FKE = \triangle LKM$, because the angles L K M, F K E, are supplementary; therefore $\square AMLB = 2\triangle AKB$; hence by (17) $\triangle AKB = \frac{1}{2}\triangle BMR$.

Construction.—Make a triangle B M R, having its sides B M, B R, equal to the given bisectors, and the angle M B R equal to half a right angle. On M R draw a semicircle, and construct a hyperbola having B M, B R, for asymptotes, and such that the rectangle under the ordinate and abscissa (parallel to the asymptotes) is half the rectangle under the given bisectors. Let this hyperbola cut the semicircle in A; join A B and produce A K parallel to B R, so that $AL = BR$; and produce B L, A M, to meet in C. Then A B C will be the triangle required.

BIBLIOGRAPHICAL NOTES AND ACKNOWLEDGMENTS.

This problem was proposed in the Ladies' Diary for 1797, by Alex. Rowe, and the following year two solutions of it were given; one by William Burdon and the other by J. Hartley. Our sixth solution is taken from Mr. Burdon, as published in Leybourn (Thomas). The Mathematical Questions proposed in the Ladies' Diary, etc., 8vo., London, 1817, vol. iii. 328.

Mr. Hartley's solution is trigonometrical, the unknown quantity being tan $\frac{1}{2}$ A, and his final equation corresponds to equation

(2), but the mode of obtaining it is not so elegant as that employed in our first solution.

The problem is proposed as an exercise in Bonnycastle (John). An Introduction in Algebra, etc., revised and enlarged, by James Ryan, 4th edition, 12mo., New York, 1829, p. 310. In the key to the second edition, New York, 1822, pp. 250–251, is a solution essentially the same as the first one given here.

The problem extended to any triangle was proposed by the writer in the Analyst, vol. iii., No. 5, Sept. 1876, p. 163, and solved in the next number, pp. 188–189, by Prof. J. Scheffer. It was also solved by Henry Gunder, William Hoover, and the writer.

The problem not extended was proposed in the Educational Times of January 1, 1879, p. 22, question 5866, by Mr. N. H. Capel; and in the following number proposed by the editor for construction, question 5885. In the May number, p. 150, a construction by Mr. R. Tucker was given, which we have here incorporated verbatim as our second construction.

For the fourth solution I am indebted to my classmate, Prof. W. W. Beman, of the University of Michigan.

At the conclusion of Mr. Baker's communication the Society adjourned.

171ST MEETING. DECEMBER 19, 1879.

Vice-President TAYLOR in the Chair.

Thirty-four members present.

The order of exercises for the evening consisted of the following communications:—

1. A paper by Prof. CHICKERING on the Luray Cave.
2. A paper by Prof. WILLIAM HARKNESS.
3. A communication by Capt. DUTTON on the Permian Formation in North America.

The paper of Mr. CHICKERING was reserved for publication.

The paper of Mr. HARKNESS was on

THE NUMBER OF LENSES REQUIRED IN AN ACHROMATIC OBJECTIVE,

consisting of infinitely thin lenses in contact, in order that, with any given law of dispersion whatever, the greatest possible number of light-rays of different degrees of refrangibility may be brought to a common focus.

For any system of thin lenses in contact we have

$$\frac{1}{f} = (\mu_1 - 1)\,A_1 + (\mu_2 - 1)\,A_2 + (\mu_3 - 1)\,A_3 + \text{etc.,} \quad (1)$$

the number of terms being unlimited. For a dispersion formula we write

$$\mu = \phi\,(\lambda). \quad (2)$$

The form of $\phi\,(\lambda)$ is unknown, but there will be no loss of generality if it is developed in a series arranged according to the powers of λ. We, therefore, have

$$\mu = a + b\lambda^m + c\lambda^n + e\lambda^p + \text{etc.,} \quad (3)$$

in which a, b, c, etc., are constants, and the number of terms may be taken as great as is desired.

Let us also put

$$\begin{aligned} C &= A_1(a_1 - 1) + A_2(a_2 - 1) + A_3(a_3 - 1) + \text{etc.} \\ D &= A_1 b_1 + A_2 b_2 + A_3 b_3 + \text{etc.} \\ E &= A_1 c_1 + A_2 c_2 + A_3 c_3 + \text{etc.} \\ F &= A_1 e_1 + A_2 e_2 + A_3 e_3 + \text{etc.} \\ &\text{etc.} \qquad \text{etc.} \qquad \text{etc.,} \end{aligned} \quad (4)$$

the number of these equations, and the number of terms in the right hand member of each of them, being the same as the number of terms in the right hand member of (3). Now substituting for the μs in (1) their values in terms of the auxiliaries C, D, E, etc., of the equations (4), we find

$$\frac{1}{f} = C + D\lambda^m + E\lambda^n + F\lambda^p + \text{etc.} \quad (5)$$

Considering λ as the abscissa, and f as the ordinate, this is the equation of the focal curve. Its first derivative, with respect to f and λ, is

$$\frac{df}{d\lambda} = -f^2\,(mD\lambda^{m-1} + nE\lambda^{n-1} + \text{etc.}), \quad (6)$$

which, as is well known, expresses for every point of the curve the tangent of the angle made by the tangent line with the axis of abscissas. The number of rays of different degrees of refrangibility which can be brought to a common focus will evidently

be the same as the number of times that the focal curve intersects the focal plane. But the focal plane is necessarily parallel to the axis of abscissas; and, therefore, the greatest possible number of intersections of the curve with the plane can only exceed by one the number of tangents which can be drawn parallel to the axis of abscissas. To find these tangents we equate (6) to zero, and obtain

$$0 = mD\lambda^{m-1} + nE\lambda^{n-1} + \text{etc.} \qquad (7)$$

As λ can never be either zero, negative, or imaginary, we have to consider only the real positive roots of this equation; each of which corresponds to a tangent. To make the number of tangents as great as possible the quantities D, E, F, etc., must be independent of each other; which will be the case when the right hand members of the equations (4) contain as many *A*s as there are powers of λ in the dispersion formula (4). All the terms of (7) contain the common factor λ^{m-1}. Taking it out we have

$$-mD = nE\lambda^{n-m} + pF\lambda^{p-m} + \text{etc.}, \qquad (8)$$

from which it is evident that the number of real positive roots in (7) will always be one less than the number of powers of λ in (3). Hence we conclude that—

In any system of infinitely thin lenses in contact, the number of lenses required to bring the greatest possible number of light-rays of different degrees of refrangibility to a common focus is the same as the number of different powers of λ contained in the dispersion formula employed.

The method made use of in arriving at this result has been adopted, because it brings out clearly the geometrical relations of the problem. The result itself is evident from a mere inspection of equation (5), which cannot possess more real positive roots than it has independent auxiliaries D, E, F, etc.

The communication of Mr. DUTTON

ON THE PERMIAN FORMATION OF NORTH AMERICA

then followed.

Mr. Dutton stated that many geologists have long been in doubt whether the Permian formation was merely of local occurrence in a very few districts constituting a subordinate series embraced within and forming a part of the closing period of the Carboniferous series, or whether it was of world-wide prevalence

and constituted a distinct period by itself. Strata of Permian age have for a considerable time been known in Kansas and in Texas, but have not been until very recently satisfactorily determined in other parts of America. There is well known to exist throughout the greater part of the mountain region of the West a series of heavy red sandstones sometimes divisible into two portions, an upper and a lower, and sometimes inseparable. The upper part of this series has been assigned with confidence to the Trias, but the lower part has not had its age satisfactorily determined, since it has not until recently yielded fossils which serve to place its age beyond doubt. During the last few months Mr. Walcott, a young palæontologist employed by the Geological Survey, has discovered in this formation, at Kanab, in southern Utah, well marked Permian fossils. The identity of the horizon from which they were taken, with the lower part of the Red beds of Colorado and Wyoming, the Uinta Mountains and New Mexico, and with the "variegated marls" of Newberry in Arizona, and New Mexico, and with the Shinarump of Powell, in the vicinity of the junction of the Grand and Green Rivers, is already proven beyond controversy. Hence this discovery establishes for the Permian in North America a general prevalence and a magnitude of development comparable with the Jurassic and Trias, and assigns it to the rank of a formation of a high order.

The meeting then adjourned

172D MEETING. JANUARY 3, 1880.

The President in the Chair.

Forty-four members present.

The minutes of the last meeting were read and adopted.

The order of proceedings for the present evening consisted in the communications of Messrs. A. GRAHAM BELL, D. P. TODD, and W. H. DALL.

The first paper was by Mr. BELL on the subject of

BINAURAL AUDITION.

(ABSTRACT.)

While in England, in 1878, it occurred to Mr. Bell that all the peculiarities of binaural hearing might be produced artificially by the telephone, as the peculiarities of binocular vision are produced by the stereoscope.

Two transmitting telephones were arranged so that the diaphragms of the instruments were about as far apart, and occupied about the same position relatively to one another, as the drum membranes of a person's ears. These transmitters were connected by two distinct and independent circuits to two receiving telephones, which were placed respectively to the right and left ears of an observer in a distant place.

When sounds were made in the neighborhood of the transmitting telephones the auditory sensations experienced by the observer in the distant place were of a decidedly novel character. The direction of the speaker's voice from the transmitting telephones could be perceived to a limited extent.

Attempts were made to have the observer determine by ear the exact location of the original sound, with the following result:—

Imagine the transmitting telephones to be placed in the interior of a globe upon which the usual meridian lines and parallels of latitude are marked so that the axis of the globe passes vertically through the centres of both diaphragms.

Now, suppose we produce a sound at some point in the neighborhood of the transmitting telephones—we can take its bearings upon the surface of our globe—we can give as it were the latitude and longitude of the sound.

It was found, as the result of a large number of experiments, that *the distant observer could tell with approximate accuracy the latitude of the sound, but that he had no idea whatever of the longitude.*

It then occurred to Mr. Bell that the telephone might be used to test whether the same law held good for direct audition.

A number of telephones were suspended in different parts of a summer-house, and were connected by independent wires to a common switch-board, so that any desired telephone could be instantly connected with a distant rheotome and battery by the operator at the switch-board.

The rheotome interrupted the battery circuit about one hundred times per second, and a loud musical note was emitted by the telephone which happened to be in circuit with it.

An observer stationed in the centre of the summer-house was required to indicate by pointing, the exact location of the telephone from which the sound proceeded. He was not allowed to

move his head, nor to open his eyes, but had to rely entirely upon the sensations produced in his ears when his head was held in a fixed position.

The bearings of his hand were taken upon an imaginary sphere in the centre of which he stood, and the reading was recorded side by side with the true place of the telephone.

After considerable experiment it was found advisable to use only one telephone, which was hung up in different parts of the summer-house during the absence of the observer—as it was found that the observer soon came to recognize each individual telephone by the quality or *timbre* of the sound produced by it —and that this recognition biassed his judgment regarding the direction of the sound.

Imagine the observer to be facing the north—then the direction of sounds produced at the easterly or westerly points of the horizon of the ears was always clearly perceived. In proportion as the angular distance of the source of sound from those points was increased the readings of the observers became wild, and when it was 90° it was not uncommon to make a mistake of 180° in the direction of the sound.

The general results of all the observations thus seem to agree very closely with those obtained by telephone; but an examination of the individual records must convince one that an individual observer discriminates the direction of a sound to a much greater extent than that indicated by the experiments first narrated. Most observers could indicate correctly the direction of sounds that proceeded from the northerly or southerly points of the horizon of the ears; few could locate a sound from the zenith, and none could tell the direction of a sound from beneath.

When a telephone was placed on the ground between the feet of the observer, and was there caused to sound—he would immediately form a mental conception of the direction of the sound, and would indicate it by pointing, but he was invariably mistaken.

Mr. Bell stated that he thought that the method pursued would ultimately lead to valuable results, but that many more experiments were necessary; and the results so far obtained he presented to the Society in a tabulated form.

TABLE I.—*Particulars concerning the persons whose hearing was tested.*

	Name.	Residence.	Age.	Distance of ear from ground.	Remarks.
Observer No. 1.	John Kelly.	Cambridge, Mass.	14 years.	4 ft. 5¾ inches.	All these observers had been taught singing at Public School by the method of Prof. Mason, and all seemed to possess fair musical ears.
" No. 2.	Charles McCourt.	" "	15 years.	4 ft. 6½ inches.	
" No. 3.	Michael Shae.	" "	15 years.	4 ft. 9½ inches.	
" No. 4.	Eugene Sullivan.	" "	17 years.	4 ft. 11 inches.	
" No. 5.	Jeremiah Mack.	" "	17 years.	?	

TABLE II.—*Relative power of the two ears as determined by Telephone for the observers alluded to in Table I.*

	OBSERVERS.				
	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Right Ear: 1st determination.	24.60 c. m.	19.00 c. m.	26.30 c. m.	30.22 c. m.	18.30 c. m.
2d "	20.10 "	23.70 "	27.20 "	29.50 "	20.00 "
3d "	22.50 "	24.96 "	23.65 "	28.86 "	20.95 "
Mean.	22.40 "	22.55 "	25.72 "	29.52 "	19.75 "
Left Ear: 1st determination.	20.13 c. m.	21.46 c. m.	20.10 c. m.	25.72 c. m.	21.30 c. m.
2d "	19.15 "	21.24 "	21.23 "	28.30 "	20.30 "
3d "	21.21 "	21.60 "	27.53 "	32.30 "	21.50 "
Mean.	20.16 "	21.43 "	22.96 "	28.77 "	21.60 "

TABLE III.—*Direction of sound as determined by right ear alone.*

	True direction of sound.	OBSERVERS.				
		No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Alt.........	0°	—10°	0°	0°	0°	0°
Az.........	0°	+23	+45	+67	0	+113
Alt.........	0	—10	45	+23	—23	—23
Az.........	+45	+67	+90	+67	+67	+45
Alt.........	0	—23	—45	+10	—23	—10
Az.........	+90	+67	+90	+90	+90	+90
Alt.........	0	—10	—45	+23	—45	—10
Az.........	+135	+90	+90	+90	+90	+90
Alt.........	0	0	0	+90	+90	—10
Az.........	±180	—45	+45	0	0	+113
Alt.........	0	0	0	0	0	0
Az.........	—135	—135	—67	—90	—90	±180
Alt.........	0	0	0	+10	+10	0
Az.........	—90	—90	±180	—90	—90	±180
Alt.........	0	0	0	+45	0	+35
Az.........	—45	—23	—67	—45	—67	0
Alt.........	+90	+90	+90	+90	+68	0
Az.........	0	0	0	0	0	±180
Alt.........	—90	—68	—23	+90	—45	—23
Az.........	0	+113	+68	0	+113	+134

TABLE IV.—*Direction of sound as determined by left ear alone.*

	True direction of sound.	OBSERVERS.				
		No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Alt.........	0	0	+10	0	—10	—10
Az.........	0	—67	—67	—45	—23	—23
Alt.........	0	0	+35	0	—10	+23
Az.........	+45	+23	+67	+67	+67	0
Alt.........	0	0	+23	0	0	—23
Az.........	+90	—135	+113	—67	+157	—135
Alt.........	0	0	—10	0	0	—23
Az.........	135	—113	—113	—67	+135	—113
Alt.........	0	0	—45	+35	+90	—23
Az.........	±180	—35	—35	0	0	—45
Alt.........	0	—10	—10	—10	—10	—23
Az.........	—135	—113	—67	—45	—45	—67
Alt.........	0	—10	0	—10	—10	—10
Az.........	—90	—113	—67	—90	—67	—90
Alt.........	0	—10	—10	—10	0	0
Az.........	—45	—45	—45	—45	—45	—67
Alt.........	+90	—10	—45	+45	+90	0
Az.........	0	—90	—45	—45	0	±180
Alt.........	—90	+90	0	—10	—45	—45
Az.........	0	0	0	—157	—135	±180

TABLE V.—*Direction of sound as determined by both ears used simultaneously.*

	True direction of sound.	OBSERVERS.				
		No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Alt.........	0	0	0	0	—10	—35
Az.........	0	0	0	0	0	+10
Alt.........	0	0	0	0	—45	0
Az.........	+45	+23	+30	+30	+67	+60
Alt.........	0	0	0	—22	—45	—22
Az.........	+90	+90	+90	+68	+90	+45
Alt.........	0	0	—10	—10	—10	0
Az.........	+135	+68	+113	+68	+90	+45
Alt.........	0	0	0	—22	—22	—22
Az.........	±180	0	+45	+90	±180	+23
Alt.........	0	—10	0	0	—10	—35
Az.........	—135	—68	—75	—90	—113	—135
Alt.........	0	0	0	0	0	0
Az.........	—90	—90	—90	—90	—90	—90
Alt.........	0	0	0	0	0	0
Az.........	—45	—67	—90	—67	—90	—67

TABLE VI.—*Direction of sound as determined by both ears used simultaneously.*

	True direction of sound.	OBSERVERS.				
		No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Alt.........	+45	0	+22	0	+90	+45
Az.........	0	0	0	0	0	0
Alt.........	+45	0	—10	+10	+45	+10
Az.........	+45	+45	+45	+45	+45	+45
Alt.........	+45	0	—45	0	+10	—22
Az.........	+90	+90	+90	+90	+68	+68
Alt.........	+45	+22	+22	+22	—30	0
Az.........	+135	+23	+68	+60	+113	+150
Alt.........	+45	0	+80	—10	+45	—45
Az.........	±180	0	+23	0	0	±180
Alt.........	+45	0	0	+10	+10	+35
Az.........	—135	—68	—68	—68	—113	—120
Alt.........	+45	—10	+10	0	+45	—22
Az.........	—90	—60	—90	—90	—90	—90
Alt.........	+45	0	0	0	+45	0
Az.........	—45	—60	—45	—90	—45	—45

TABLE VII.—*Direction of sound as determined by both ears used simultaneously.*

	True direction of sound.	Observers. No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Alt	—45	0	—10	—10	+80	—45
Az	0	0	0	0	±180	0
Alt	—45	0	—23	0	0	—10
Az	+45	+45	+45	+90	+113	+45
Alt	—45	0	—23	0	—23	—45
Az	+90	+75	+60	+68	+90	+68
Alt	—45	—23	0	—10	—45	—35
Az	+135	+105	+75	+90	+120	+120
Alt	—45	0	0	—10	—45	—45
Az	±180	+23	±180	+128	—135	±180
Alt	—45	—30	0	—10	—10	—23
Az	—135	—135	—60	—105	—135	—113
Alt	—45	—35	—23	0	—45	—45
Az	—90	—90	—68	—90	—90	—90
Alt	—45	—10	—45	+45	+10	0
Az	—45	—45	—45	—68	—45	—45*

TABLE VIII.—*Direction of sound as determined by both ears used simultaneously.*

	True direction of sound.	Observers. No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Alt	+90	+45	+23	0	+90	+45
Az	0	0	0	0	0	±180
Alt	—90	0	+90	0	+90	+90
Az	0	+22	0	0	0	0
Alt	—90	0	—23	+10	+45	—23
Az	0	0	—45	—23	+90	+68

Mr. D. P. Todd's paper was entitled

SOLAR PARALLAX FROM THE VELOCITY OF LIGHT.

(*This paper will be found published in full in the American Journal of Science for January*, 1880.)

* This observer was uncertain whether the sound came from a point of the horizon 45° to the left of the zero point or from one 135° to the left. After having the sound repeated a number of times he decided upon the former direction.

The next communication, by Mr. W. H. DALL, was

SOME RECENT OBSERVATIONS ON MOLLUSCS.

1. He remarked first that he had observed in a species of *Buccinum* (*B. undatum*, L.) that the males were much smaller than the females. This species is found upon rocky coasts exposed to the action of waves and surf; and the animals in such situations are usually found inhabiting crevices in the rocks, where they find protection and shelter from the violence of the water. In more exposed situations they would not be able to survive. Prof. E. S. Morse had published the suggestion that the small size of the males might be due to a special action of the natural law of survival of the fittest. In the contracted crevices of the rocks, the conjunction of the sexes would be much facilitated if the males were considerably smaller than the females, and would be much restricted if both sexes were large. Hence, under such circumstances, the males would oftenest succeed in obtaining access to the females, and would oftenest propagate, the result being a tendency to diminution of the males. Mr. Dall was of the opinion that this explanation was not sustained when brought into relation with other facts in connection with habits of the many species of *Buccinum* in which the diminutive size of males is a common fact and is prevalent in species which inhabit still waters and other places where access to females cannot be dependent merely upon the small size of the male. He was of the opinion that a more satisfactory explanation would be found in the fact that in marine animals great fecundity is necessary to perpetuate the species, and that in order to nourish the very numerous ova, large ovaries, large organs of nutrition, and, in general, largeness of the entire organism is requisite in the females, without any corresponding necessity in the males.

2. Mr. Dall next referred to some observations by Mr. R. P. Whitfield, of New York, upon some individuals of the species *Limnæa megasoma*—one of the largest known species of that genus. The animals were kept in a small tank and propagated. In the course of several generations a conspicuous diminution in the size of the individuals was observed. Mr. Whitfield had merely stated the observation without suggesting the explanation. Other naturalists, however, had suggested that it might be attributed to the higher temperature of the water in the tank than that of the water which the animals naturally inhabited. Mr. Dall thought that a much better explanation was that these *Lim-*

næas, which are very voracious, could not find their accustomed amount of food in such a restricted habitation, and were reduced in size in consequence of their half-famished condition. He cited other observations upon the lower animals which tended to confirm the belief that insufficient food, through successive generations continuously, dwarfs the individuals of a species.

He next remarked upon a characteristic of the genus *Pleurotomaria*. This species of this genus all have a notch in the aperture of the trochoid shell, and in some species this notch is very deeply incised. The function of this notch in the economy of the animal has been a matter of some doubt. Mr. Dall believed that its use was to permit the rejection of fecal products when the animal is retracted into its shell. In many gasteropods the anus is located in the anterior part of the soma, while in the *Pleurotomaridæ* this orifice is located behind the gills, and would be covered by the shell when the body is retracted were not a special modification of the shell-aperture provided.

At the conclusion of Mr. DALL's communication the Society adjourned.

173D MEETING. JANUARY 17, 1880.

Vice-President WELLING in the Chair.

Forty-one members present.

The minutes of the last meeting were read and approved.

The order of exercises for the evening consisted in the presentation of communications from Messrs. M. H. DOOLITTLE and W. H. DALL.

Mr. DOOLITTLE's subject was

A PILE OF BALLS.

(ABSTRACT.)

Mr. Doolittle's communication was a discussion of the appearance the sky would present if the stars were of equal absolute brilliancy, with a relative arrangement in space corresponding to the centres of balls in a regular pile. The triangular and the rectangular pile differ in respect to anterior only. The most convenient Cartesian co-ordinate axes consist of the diagonals of a square base with a perpendicular thereto. The most convenient unit is equal to radius multiplied by the square root of 2. Then, the origin being at the centre of a ball, the centres of all

other balls are determined by the conditions that each co-ordinate shall be an integer, and that the sum of the co-ordinates shall be an even number. The distance in diameters of any ball centre from the origin is equal to the square root of half the sum of the squares of the co-ordinates.

In the most general case, the permutation of the co-ordinates gives the factor 6; and as the algebraic sign of each co-ordinate may be either positive or negative, the number of variations for each permutation is equal to 8. The product 48 is the number of balls having the same distance and symmetrical arrangement. In particular cases two or all three of the co-ordinates may be numerically equal, or one or more of them may be equal to 0, and the number of such ball centres may be reduced to 6, 8, 12, or 24. Since a number may be the common sum of different sets of squares, there may be more than 48 equidistant balls belonging to two or more independent symmetrical arrangements. Thus 50 is the common sum of 9, 16, and 25; of 0, 25, and 25; and of 0, 1, and 49; and there are in all 84 ball centres at the distance of 5 diameters from the origin.

The co-ordinates 0, 1, and 1 give 12 tangent balls, or stars of first magnitude in the imaginary universe; 0, 0, 2 give 6 of 2d magnitude; 1, 1, 2 give 24 of 3d magnitude; 0, 2, 2 give 12 of 4th magnitude; 0, 1, 3 give 24 of 5th magnitude; 2, 2, 2 give 8 of 6th magnitude; 1, 2, 3 give 48 of 7th magnitude, etc.

If the origin be regarded as at the centre of a cube whose faces are perpendicular to the co-ordinate axes, the 12 stars of 1st magnitude are in the directions of the middle points of the edges, and the 12 of 4th magnitude in right lines beyond those of 1st magnitude; the 6 of 2d magnitude are in the directions of the centres of the faces; and the 8 of 6th magnitude are in the directions of the corners.

The formula A, b, c may appropriately represent one large co-ordinate and two small ones; and the corresponding constellations consist of octagons around the face-centres, becoming squares when b is numerically equal to c, or when either is equal to 0. The formula A, B, c, denoting two large co-ordinates and one small one, corresponds to rectangles about the middle points of the edges, becoming pairs when A is numerically equal to B, or when c is equal to 0. The formula A, B, C, denoting co-ordinates nearly equal, corresponds to hexagons about the corners, which are regular when A, B, and C are numerically in arithmetical progression, and become triangles when two of the co-ordinates are numerically equal.

Mr. DALL's communication On the Boundary Line between Alaska and British America, having been made on the spur of the moment to fill an unoccupied hour of the evening, he desired no further mention than the entrance of the title upon the minutes of the meeting.

The Society then adjourned.

174TH MEETING. JANUARY 31, 1880.

The President in the Chair.

Forty-four members present.

The minutes of the last meeting were read and adopted.

The order of exercises for the evening consisted in the reading of a paper by Capt. C. E. DUTTON on

THE SILVER QUESTION.

BY the Act of February 28, 1878, the Secretary of the Treasury is required to purchase monthly not less than two million nor more than four million dollars' worth of silver, and coin the same into dollars, each dollar weighing 412½ grains, and being nine-tenths fine. The dollars so coined are declared by the same law to be legal tender, in any amount, for all debts public and private, including duties on imports and interest and principal of the public debt. The Secretary of the Treasury is also authorized and required to issue certificates, payable on demand in silver dollars, provided that the amount of certificates so issued shall not be in excess of the silver coin deposited in the Treasury, and the denominations of the certificates shall not be less than ten dollars. These also are legal tender to the same extent as the silver dollars themselves.

There are many careful thinkers who believe that the passage of the law of February 28, 1878, was ill-advised, and that it should be repealed, or radically modified. These objections are met by the reply that the country is now in a highly prosperous condition, and it would be best to "let well enough alone." To this the rejoinder is, that it is certainly best now and always to let well enough alone, but this aphorism, like many others, is merely a curt way of begging the question. The real question which is raised is, whether the present silver law is well enough; if so, then by all means let it alone; if not, then it should be reconsidered, and either amended or repealed. The object of this paper is to inquire whether the silver law is well enough to remain upon the statute book, or ill enough to demand a reconsideration.

Among the causes which tend to promote prosperity is a wise system of monetary laws. Among those which tend to bring adversity is an unwise system of monetary laws. The wise system, however, will not alone insure prosperity, though it may go far towards it, and prosperity may come and abide with us for a season in spite of the bad system which will make itself felt only in the long run by making the adversity, which is sure to follow, the more severe. If the silver law be a bad law, therefore, we need not expect to feel its effects sorely during the flood tide, but we cannot hope to escape its pressure during the ebb. If it be a bad law the time to amend it is most assuredly

during the period of prosperity. To amend it undoubtedly will involve some sacrifice, but a sacrifice made to secure immunity from the consequences which must inevitably follow the omission to do the right thing at the right time. It is better to do this out of our plenty than to wait in order to do it, or pay the penalty of not doing it out of our poverty and adversity. If it be a bad law, then the policy of "letting well enough alone" is as wise as the man we have so often heard of in the story of the Arkansas Traveller, who would not mend his roof in fair weather because it give him no trouble, and who could not mend it in the rain without getting wet and taking cold.

In comprehending the merits of the law of February 28, 1878, we shall be materially assisted by taking a brief review of the history of our metallic currency. And this is all the more essential since the arguments which were employed to sustain its passage had recourse to this same history, and the dollar which that law ordained is claimed to be the dollar of our fathers, and to embody a return to the policy, which originated from the wisdom of the founders of the Republic, and which has prevailed without interruption throughout the eventful and glorious years of our adolescence and early manhood, until it was interrupted by a stealthy device sprung upon an unsuspecting Congress, and passed without anybody, except its authors, being aware of its dangerous nature and iniquitous purpose.

At the close of the War of Independence one of the earliest subjects to engage the attention of the statesmen and legislators, who were laying the foundations of a great nation, was a national coinage and legal tender. The subject was a difficult and delicate one. In the several States there was a sufficiently good system of weights and measures, which was uniform throughout all of them. A bushel, a quart, a pound avoirdupois meant the same thing from New Hampshire to Georgia. But with money it was very different. The only coins in use were, for the most part, foreign, of many kinds, and a few colonial coins. In the moneys of account the same terms were applied to coins or nominal moneys with widely different significations and values. Four shillings might signify in New Hampshire the same thing as twenty to twenty-two shillings in Georgia, provided a definite understanding could be reached as to the kind of coin to be used in a given transaction, and discordances only a little less wide existed between moneys of the same name used in other States. While all of the nations with which the new States held traffic had comparatively respectable coinages—some of them very good ones—America had none, and became the dumping ground of debased and abraded coins swept out of the circulation of richer and more populous nations. There was no legal tender, and it was necessary for contracts to specify in what kind of foreign coin they should be paid, and should any foreign State see fit to debase its coins the creditor, unless otherwise specially protected and guar-

anteed, might be liable to suffer by the debasement. The necessity for a national coinage was apparent, but the difficulties in the way of establishing it and making it practical were very considerable, and required the greatest wisdom and care in the framing. The leading statesmen and financiers of that day fully appreciated them, and displayed great wisdom in dealing with them. A careful perusal of the State papers of Robert Morris, Jefferson, and Hamilton will show that, so far as the general principles of monetary science are concerned, their knowledge would compare favorably with that of the ablest financiers of the present. In truth, they adopted, as fundamental principles, precisely the same generalizations and formal laws as those which are now accepted by standard writers and eminent statesmen, who mould the opinions and direct the monetary policy of Europe and America.

The three eminent men just mentioned were at different times called upon officially to propound a monetary policy, and all expressed similar views in relation to coinage. The principles which they laid down may be summarized substantially as follows:—

1. Gold and silver are the most suitable of all known substances for use as money.

2. In selecting a unit of money it was important to choose one which was well known to the people, and assimilated as nearly as possible to one common unit in general use throughout the entire country.

3. Gold is most suitable for large money transactions, and silver for the commoner and smaller transactions.

4. Whether both metals could be used with unlimited legal tender power, would depend upon certain conditions which could be determined only by actual experiment. It was recognized that both metals had their market values, which could not be controlled or modified by legislation; that an ounce of gold was worth between $14\frac{1}{2}$ to $15\frac{1}{2}$ times as much as an ounce of silver; though the exact ratio, even if it existed, could not be ascertained.

5. If it were desirable to attempt to maintain a coinage of two metals with unlimited legal tender power for both, and with the same denominations, it would be necessary that the gold unit should have precisely the same value as the silver unit; for, if the value of the one were less than that of the other, then the less valuable one would alone be coined and would monopolize the circulation.

6. But even if the coinage were so regulated that the gold and silver units should for the time being have equal values, this equality is liable to disappear in consequence of changes in the relative market values of the two metals; and whenever such a change occurs the depreciated metal would alone circulate.

7. The most desirable characteristic of silver and gold "our fathers" believed to be the steadiness of their values. They were

aware that they fluctuated somewhat—gold varying less than silver—but such statistical and historical knowledge as they could procure led them to believe that the fluctuations were very small and took place with great slowness. This attribute was a most estimable one in their opinion; for, they recognized that a metallic currency endowed with legal tender power could not change its absolute or relative value without modifying the obligations of contracts. If the precious metals rose, the creditors would unjustly gain, and the debtors unjustly lose: if they declined, the debtors would unjustly gain, and the creditors unjustly lose.

8. They believed, on the whole, that the "double standard" would be, for a time at least, practicable. They believed that the great steadiness of the two metals would enable them to circulate together for a considerable time, and if after the lapse of years a change should have developed in their relative values, their descendants would perceive as readily as themselves, the nature of the difficulty, and apply the only possible remedy.

As a matter of fact, our fathers apparently believed that both gold and silver should be maintained in the coinage with unlimited legal tender power. It is a little remarkable that they nowhere discuss the relative advantages of a bimetallic and monometallic coinage. Probably it never occurred to them that a question of this nature could arise, and took it for granted, as all nations up to that time had done, that silver was indispensable, and gold extremely desirable, and hence that both should be used as unlimited legal tender. It is not difficult, however, to suggest good reasons for this state of mind. Very few people at that time were rich enough to be guilty of the extravagance and risk of carrying such a large sum as ten dollars in their pockets except upon rare occasions. Large cash transactions were much less common than now, and small ones comparatively numerous. Gold coins would have been inconveniently large in their denominations, or dangerously small in size for the grocery and country store. For a very poor people silver is unquestionably a far better and more convenient currency than gold—or rather, it is better for the primary currency, while gold is of secondary advantage. Moreover, during those few intervals in which a coin currency had taken the place of paper, the people had been accustomed to pass silver coins by tale, while gold, on account of its great value, had always passed by weight. They were far more accustomed to silver than to gold, and depreciated paper was probably more familiar than either. In general the poor people of nearly every civilized nation at that time used silver as their principal metallic currency. It is not at all surprising, therefore, if our fathers never thought of limiting the use of silver as money.

In selecting a unit there was almost universal approbation of the idea that it should conform as nearly as possible to the Spanish dollar or "piece of eight" reals. Coins of many nationali-

ties were in common use. The English denominations of pounds, shillings, and pence were everywhere employed, but with widely varying values. There was one coin alone which was everywhere understood, and which meant the same thing "from Maine to Georgia." The Spanish dollar alone fulfilled the requirement of a coin to which all the people of the States were accustomed. There was, however, some difficulty in determining just what amount of silver this coin contained. Careful weighing showed that pieces of the same date and mint varied considerably in value. It was well known that the quantity of silver had been intentionally reduced within half a century, the dollar having contained in 1735 about 387 grains of fine silver, and subsequently 374, and then 368 grains, as nearly as could be ascertained. In fixing the coinage in 1792 it was after long discussion enacted that there should be struck at the mint silver "dollars or units, each to be of the value of a Spanish milled dollar as the same is now current, and to contain $371\frac{4}{16}$ grains of pure, or 416 grains of standard silver." The fractional coins contained silver in the same proportions to their nominal values. By the same act the quantity of gold in the ten dollar piece was fixed at $247\frac{1}{2}$ grains fine, and 270 grains standard. Hence the ratio of the quantity of gold in a gold dollar to the quantity of silver in a silver dollar, was about as 1 to 15. The coinage commenced in 1793 and continued until the close of the year 1803, and the total number of silver dollars struck was less than 1,500,000. From 1804 to 1840 no silver dollars were coined, with the exception of a few proof specimens. The coinage of half dollars, however, was much greater; but when their number relatively to population is considered, it will still appear very small; and the same is true of the gold coinage of this period. The reason is not far to seek. Both gold and silver were driven out of circulation by paper money.

It is impossible to gain a correct idea of the financial policy of our government with respect to coinage and of the behavior of our metallic currency without taking into consideration the salient facts of our history relating to paper money. It must be confessed that the American people, since early colonial times, have displayed a most remarkable predilection for bad currency. In every other civilized nation the evils of irredeemable and redundant paper have for a century and a half been well understood and fully acknowledged. A resort to it has never been justified except by speculators and communists, unless as a forced and deplorable alternative in time of war. The nations which have adopted it have felt keenly its evils, and, smarting under them, have struggled to emancipate themselves from them. Americans alone have refused to take warning by bitter experience, and have again and again betaken themselves to this device, sometimes with an excuse but more frequently without a shadow of a pretext. It has become a firmly settled national habit. Peo-

ple are accustomed to it so thoroughly that they have contracted the consequent habit of preferring the very conditions of insecurity, reckless waste, and instability of all things financial, to solid and normal methods of business. As early as 1690 Massachusetts began the issue of bank paper in irredeemable quantity, and her example was quickly followed by other colonies. As fast as one issue depreciated into worthlessness it was followed by another. Hardly an act of recklessness and folly which can be attached to such a measure was omitted. The issues were accompanied by acts making them a legal tender and a forced circulation, and were followed by arbitrary and even cruel laws designed to regulate their value. Penalties were fixed for refusing them or for depreciating them, and these were harsh and even outrageous. Numberless persons were robbed and ruined without knowing how or why, while others grew suddenly rich, and it is needless to say that the ones who suffered were those who had before been honest, industrious, and thrifty, and those who prospered were chiefly those who had speculated and cheated successfully. The hard-earned savings of the many were transferred to the pockets of the few. And then came complaints and outcries from thousands who had been victimized and who clamored for redress. Yet the people who had been plundered were the very ones who, by their votes and through their delegates to the colonial legislatures, had effected the paper issues and ordained the instrumentality by which they had been ruined against the protests of wiser and richer men and over the vetoes of governors who understood and vainly prophesied the disastrous consequences. When the people had reaped the crop they had sown and tasted its bitterness, the remedy which they demanded was more paper money, and more was issued. The result was perfectly natural. It gave at first some apparent relief, which was quickly followed by distress greater than before. After several repetitions of this indulgence "our fathers" began to realize the true cause of the difficulty and attempted to recover lost ground, but found that the downward road was much easier to travel than the upward. Good resolutions gave way before difficulties, and as national calamities are soon forgotten or only vaguely remembered, and as the sons are ever ready to repeat the errors of their fathers, the intervals of abstinence were generally short, and were followed by a relapse into the old vice. Issue followed issue in dreary succession. Every scheme for "basing" bank issues which ingenuity and cupidity could devise to tempt people to accept paper was resorted to, each new plan professing to avoid the errors of its predecessors and to rest upon a secure foundation. At length, in 1751, the representations of the colonial governors and substantial merchants were effectual in securing an act of Parliament prohibiting all paper issues except exchequer bills redeemable in one year with interest, or in four years in case of war, and the colonies made a more serious

attempt to return to specie payments. Some progress had been made, when war with France broke out, and the printing press furnished the ways and means. Fortunately, the large ransom paid by the French for the return of Louisbourg put Massachusetts in possession of a considerable sum of silver and gold, with which that colony was enabled to buy up her paper at about eleven for one, and base her transactions for a time thereafter upon specie. The interval of specie payments, however, was a short one; but it was full of instruction, and inculcates a lesson which *mutatis mutandis* may be pondered with great advantage at the present time.

"Scarcely had specie come into circulation in Massachusetts when it was found that, although the remittance had been in silver, gold from the West Indies began to stay in the colony. The question of making it legal tender as well as silver soon began to be agitated. It circulated of course, not being legal tender, at its weight. An act was passed in 1762 to make gold legal tender at $2\frac{1}{2}d.$ per grain. At this rate it was more profitable for the debtor to pay in gold than in silver. The [silver] currency was depreciated five per cent. by this operation, and, as Hutchinson declared at the time must follow, this drove silver out of circulation. Some hints also show that barter currency was still allowed in the payment of taxes. Silver now became scarce, and the next stage was a new agitation in 1767 for paper money." [Prof. W. G. Sumner, History of American Currency.]

For several years the agitation in favor of paper money in Massachusetts was not successful. But the other colonies were flooded with it, and when the war of the Revolution commenced, Massachusetts was forced to admit the circulation of notes from other colonies. The principal facts connected with the issue of paper by the Congress of the Revolution are familiar to all. When the war came there was scarcely any other currency in use in the colonies; the specie had flown away to other countries, and there was apparently no other resource. How far this proceeding was wise it is difficult at this time to judge. It seems certain, however, that nothing less than utter desperation can ever warrant such extravagant use of this expedient as was made at that time. The needless suffering and cruel injustice which it inflicted upon the very class which was most patriotic and deserving passes all estimate. In 1780 it ceased to circulate from sheer worthlessness, and the scanty supply of coin which had been hoarded during the paper deluge came forth and took its place.

In 1781 the Bank of North America was chartered at Philadelphia, which issued notes redeemable in specie. This bank appeared to be solvent, but people had become so distrustful of paper money that it found difficulty in circulating its notes, until a long period of maintenance of specie payments slowly created confidence. The example gave rise to other banks of similar

nature which issued notes which were redeemable—at least professedly—in coin at sight. The Federal Congress refused to recharter the Bank of North America in 1788, and similarly refused to charter any other bank, but the State legislatures supplied this deficiency. There was a notable difference between this mode of sustaining a paper circulation and that which had prevailed in the colonies. These bank issues were professedly at least, and at the outset, undoubtedly, based upon coin redemption on demand. The colonial bank-issues were irredeemable and based "on wind." But the State bank system had from the beginning inherent defects which in time proved to be the source of evils only a little less serious than those which had beset and disgraced the colonial system. There was no maximum limit to the amount of notes which they might issue, and no minimum limit to the amount of coin which they must keep on hand for redeeming the notes. These limits rested wholly upon the discretion and honor of the bankers, and the willingness of people to accept and pass the notes, and refrain from presenting them. The result was that many banks pushed their circulation to the utmost and reduced their specie to the lowest limit they dared to. Many banks collapsed, leaving millions of worthless notes in the pockets of the people, and the credit of the best banks suffered by reflection.

In the mean time it is difficult at present to ascertain with all desirable exactitude the part which gold and silver performed in the currency of the country during the first quarter of this century. There were very many banks in the New England and Middle States which were managed by as sound and conservative a policy as the general system would admit of. In that portion of the country the disease of a rotten currency had greatly abated and the malady was transferred to the Southern and Western States. Coin was used in the New England and Middle States in notable quantity, but paper was still the chief circulation. The use of coin at all events was sufficient to render potent and oppressive an evil which had been growing ever since the first coinage act in 1792, and which had reached serious proportions in 1820. There was very little gold in the country, and very little came in from abroad. When the balance of trade was in our favor the balances were remitted chiefly in silver. When it was against us a great demand arose for gold to settle it, and very little gold was procurable. To financiers the cause oft he trouble was perfectly obvious. A hundred dollars in gold had by law in America the same debt-paying power as a hundred dollars in silver. In Europe $100 in gold would pay as much indebtedness as $106 in silver. No merchant would send to Europe, if he could help it, $106 when $100 would do equally well. Nor would an Englishman send £106 worth of gold to America when £100 worth of silver would be just as good. Hence we sent to Europe all the gold we could muster and

Europe sent back silver. The want of gold was a real want. It was needed to pay domestic exchanges. The extent of monetary transactions had so much increased that silver was too bulky and inconvenient, not only for the gross transactions and clearances, but even for many of the smaller local settlements of accounts. An agitation arose for the restoration of gold to the currency, and waxed stronger and more clamorous. Nothing, however, was accomplished by legislation until 1834. The proper remedy for the scarcity of gold was easy to discern.

When the first coinage act of 1792 was passed, its intention was to make the bullion values of the gold and silver dollars equal to each other, and also equal to the old Spanish dollar as then current. The projectors and authors of that act were fully aware that with the lapse of time the bullion values of the two might diverge, and the result would be that the metal of lower value would monopolize the coin circulation. But they presumed that their successors would judge for themselves whether both standards were necessary or advantageous, and would, if they desired both, again equalize their values by altering the coinage to conform to the changed market values of the two metals. In this they were justified by the action of Congress in 1834. The expedient required for bringing back gold must obviously be either to diminish the quantity of gold in the gold dollar or to increase the quantity of silver in the silver dollar. But of which metal should the rating be changed?

It is well to look carefully into this question, for it involves far more than appears upon the surface. The answer to this question *as applied to* 1834 is that the rating of gold should be changed and that of silver preserved. The reasoning upon which this answer is founded is as follows: Silver was at that time, "by a large majority," the coin in daily use. It had been so for fifteen years. Most of the obligations which had been incurred involving the payment or receipt of money, so far as they involved the functions of coin, meant silver coin. People so understood and accepted. If dollars were spoken of, they meant silver dollars or bank paper redeemable in silver dollars. Gold was out of the question, and banks and merchants never dealt in gold except with an eye to foreign exchange. To have altered the rating of silver, therefore, would have altered every time obligation and every form of valuation. It would have been a virtual violation of the constitutional provision which prohibits the passage of any law impairing the obligation of contracts. On the other hand, no such difficulty or evil attached to a change in the rating of gold. Possibly a few contracts might have been running specifying gold coin as the medium of payments, but they could have been but of small magnitude in comparison with those which were payable implicitly or explicitly in silver or its equivalent. Hence an increase in the rating of gold, or,

in other words, a diminution of the quantity of gold in the dollar was resolved on and became a law.

Three years later, in 1837, a revision of the coinage laws was made in which the quantity of silver in the silver dollar was slightly increased. The act of 1792 provided for 371¼ grains of fine silver and 416 grains standard; while the act of 1837 made it 375 grains fine and 412½ grains standard, which is precisely the same as the present silver dollar. The last-named act made no material change in the gold coinage from the act of 1834. Hence subsequent to 1837 the mint ratio for the values of the two metals was 1 : 16, *i.e.*, the legal tender power of an ounce of gold was made equal to that of 16 ounces of silver very nearly. It is now very generally conceded that this ratio overvalued gold somewhat and undervalued silver, and its effect was very speedy. In a few years silver coin became very scarce, while gold became as abundant as could be expected in a country where the chief currency was paper. So far as coin became a basis or standard of valuation gold at length took the place which had formerly been occupied by silver. Very soon after 1837 we became a monometallic gold-using nation by virtue of these changes, though at what precise period it is difficult to say, but as nearly as can be now inferred, the change took place about 1840 to 1842.

The discoveries of gold in California and Australia introduced an important element of disturbance into the relations of gold and silver. As soon as it became manifest that these rich mineral districts would for an indefinite period yield large quantities of gold, all students of monetary science foresaw that one effect would most probably be a gradual fall in the price of that metal relatively to the price of silver. These anticipations were quickly verified, and in the course of a few years the value of gold bullion rated in silver had evidently depreciated. Besides this cause, which was of world-wide operation, there was another cause at work in America which tended to make silver comparatively scarce, and the silver coins in circulation of inferior quality. Ever since colonial times Spanish coins had been extensively used, and the coinage acts of our government had made them unlimited legal tender. The rating of silver coins in other countries had become such that America was an exceptionally good market for them, and they circulated freely for change. They became much abraded, and no effort had been made to call them in and recoin them. The national fractional coinage was of full weight, and also unlimited legal tender. The abraded condition of the Spanish coins rendered them of inferior value, and by the well known law they displaced the better coinage of our own country. By the act of 1853 the fractional silver coins were debased. The silver dollar, however, was retained at its full value, 412½ grains standard and 375 grains fine, with unlimited legal tender. The Spanish coins were demonetized in 1857 and under-

rated, and, of course, they quickly went into the melting pots. The object of debasing the fractional coins was to keep them in the country as money—to prevent their being exported or melted, and avoid the expense of coining more to replace those which might be thus treated. A necessary part of this plan was to limit their legal tender to small sums, and that the government should reserve to itself the right to have them struck and monopolize the profits arising from the debasement.

The effect of the law of 1853 was simply to establish and introduce a very good fractional currency in the place of a bad one. It had no other effect, and it does not appear that any other was contemplated. Probably the real reason for leaving the silver dollar in *statu quo* was that the higher and more inexorable laws of trade had already taken care of it. These laws had expelled it, and the legislators of 1853 did not see fit to attempt to recall it. The logic of the Act may be summed up in a very few words. Silver money—both dollars and fractions—had substantially gone out of coinage, and Congressmen knew perfectly well the reason why. They recalled the fractional coins, because they wanted them. They omitted to recall the silver dollars because they did *not* want them.

The coinage of silver dollars from 1840 to 1873 was only $6,595,021, and from 1793 to 1873 the total number coined was $8,045,838. Of these nearly the whole were exported to the silver using countries, and it may be said with exactitude that the silver dollar never formed any appreciable part of our domestic circulation. Its enumeration in the list of coins of unlimited tender was practically a dead letter subsequent to 1834, and probably from a still older date. Gold had completely supplanted it, and silver had logically and by the force of circumstances taken the place of a subsidiary currency with a limited legal tender.

In 1873, when the prospect of specie payments seemed to take definite shape in the not distant future, the subject of a revision of the coinage laws came up as an essential step in the gradual progress towards that consummation. For eleven years the sole money of the country in use for domestic purposes was made of paper, except in California, Oregon, and Nevada. But for foreign exchanges, for interest on the public debt, and for duties on imports, coin had been used, and, it is needless to say, gold coin only or bullion. Silver had disappeared entirely from use, except for change in those limited transactions. To whatsoever extent coin was used, the only coin was of gold. Premiums were gold premiums. The measure of foreign exchange was a golden measure. Silver had dropped so completely out of sight that it is probable that not one person in a thousand in the country knew certainly that a silver dollar was a legal possibility with unlimited legal tender. Probably not one person in a hundred then living had ever seen one within twenty or thirty years.

A few merchants in California and New York, however, had occasion to use small quantities of these dollars for export to China, and to a very small extent to some of the small colonies of the West Indies, and even to South America. Hence the mint reports show the coinage of a few hundred thousand silver dollars each year from 1841 to 1873. In revising the coinage laws it was considered advisable to omit the old silver dollar from the list of coins and substitute the trade dollar, which was slightly heavier and more valuable, and which would be received much more freely by the Chinese, thus giving a good market for some of the silver from the western mines. The new trade dollars were made legal tender only to the amount of five dollars, and thus the silver dollar as money of unlimited tender, and as a standard of value, disappeared from the statute book.

The amount and energetic character of the denunciation which has been fulminated against the authors of the action which definitively abolished the silver dollar is something amazing. Never was denunciation more unreasonable and unjustifiable. It was dropped because it was obsolete; nobody had any use for it except a few merchants dealing with China; and for their especial accommodation the trade dollar was provided and proved to be a much more desirable article. Its omission from the list of coins had at the time no more significance than the omission to provide for any other thing which had ceased to be functional. It was said to have been demonetized by stealth. That it attracted no attention at the time is doubtless true. Why should it have attracted attention? It had no more importance as to existing facts than the omission to provide for the coining of old Hebrew shekels or Roman sesterces. It is said that it "took away a right and a privilege." What right; and what privilege? Was it the right and privilege of sending silver to the mint to be coined into dollars worth three per cent. less as money than as bullion in order to pay debts with them? A man who wanted such a privilege or demanded such a right would then have stood in need of a supervisor. The mints had been open to the world for forty years, and it is not probable that a single human being had in that time sent a pound of silver there to be turned into dollars for purposes of legal tender. No—: this was not what was desired. The privilege which was withheld was the privilege of paying with silver, debts which had been contracted in gold, after silver had fallen from twelve to twenty per cent. in value. The equity, morality, and policy of attempting to enforce such payments will be discussed presently.

The fall in the gold price of silver within the last six years is undoubtedly the most remarkable event in the history of the precious metals. The causes are now tolerably well known, viz: the demonetization of silver by the European nations, the inability of India to absorb its usual annual supply, and the greatly increased production of the American mines. It is claimed by

some that gold on the contrary has absolutely risen in price. The causes which have occasioned the fall of silver have, it is urged, been inverted as to gold. The change from silver to gold in Germany and Scandinavia has created an abnormal and great demand for the latter metal, and mining statistics indicate a diminution in the gold yield of America and Australia when the last ten years are compared with the preceding twenty. It seems very probable that gold has actually risen. It is, however, extremely difficult to say how much even very approximately, and an exact determination of the amount of fluctuation is impracticable. On the whole, the amount of change in the value of gold has probably been small while the absolute depreciation of silver has with equal probability been very great, being represented by much the greater part of the difference between the former premium over gold and the present discount, *i. e.* between 103 in 1869 and 87½ in 1880.

The Act of February 28th, 1878, must ever be regarded by students of money as one of the strangest and most unjustifiable actions ever performed by a legislative body in dealing with monetary affairs. A century ago almost any nation might have committed an extravagant financial blunder. Sixty years ago any nation excepting England, France, and Germany might have fallen into serious error about its money. But that a nation having so much wealth and intelligence as the United States should have rushed pell mell into such a trap in the last quarter of the nineteenth century without the slightest pretext passes comprehension. The Senators and Representatives of Congress taken as a body are a class of able men, worthy of the great nation they represent. They passed this act in response to what seems to have been an overwhelming movement and sentiment coming from the masses of voters, and probably felt, independently of their own convictions as to the merits of the question itself, that they were in duty bound to vote upon it in conformity with the views of their constituents. Most of them however, probably accepted the popular view of the subject and believed in it. Yet, that view is antagonized by nine-tenths of the students of money and financiers of Europe and America. Any writer upon the subject must discuss it with a profound deference for the opinions of our "conscript fathers." Yet in the two years which have elapsed since that bill was passed, it has been subjected to careful analysis and criticism, and the general bearing and effects of the measure have been made much clearer than they were then. The result of that criticism I believe to be a clear showing that the act remonetizing silver was unwise, impolitic, and unless reversed, is now fraught with great impending evil to the country.

The arguments used to sustain the measure were numerous. The most conspicuous one held up to the οἱ πολλοὶ was the catchword of "the dollar of our fathers." It would be a needless

insult to charge any congressman with believing that the use of any particular dollar by our great-grandfathers was a reason for retaining it if the intervening period had brought into use a better and more convenient substitute. We might as well use the same argument for re-arming our troops with blunderbusses and revert to travelling by stage coaches. The dollar of our fathers was a very protean thing, and by far the commonest form it took was that of paper, the filthiest form of filthy lucre. The value which they themselves put upon it is best illustrated by recalling the names they gave it, "wild cat," "red dog," "coon box" money. Among the many forms of fossil dollars that of 1837 was selected.

Another argument which was used was that the public debt of the United States consisted of bonds, which were sold for paper money at a time when coin was at a very high premium, and that if they were repaid even in depreciated silver, the bondholders would receive far more than the first purchasers paid for them; whereas strict equity would have been more than satisfied if they were paid in paper. It was admitted, however, that the Act of March 18th, 1869, declaring that the faith of the United States was solemnly pledged to their payment in coin, settled the matter so far as paper was concerned. This resolution was asserted to have been unwise and unjust, and framed in the interest of bondholders at the expense of poorer tax-payers; but it must be recognized as valid. But it was still left optional with the Government to pay its bonds in silver, because the silver dollar was a lawful coin of unlimited legal tender when that resolution was passed, and the Government would be unjust to itself and to its tax-payers if it did not take advantage of the option.

This argument does not fully and fairly state the premises, and is very wide of the mark in its conclusion. Whatever option may have remained in 1869 was swept away by the coinage act of 1873 leaving gold as the only lawful medium of payment. That option was abandoned because gold was, and for thirty years had been, the cheaper metal, and the only metal used for unlimited coin payments. It was abandoned because it appeared to be valueless. Nobody objected. The bondholders were satisfied because they regarded their bonds payable in gold, and expected nothing better or worse. The Government, which owed the payments, could not help being satisfied, because it had always expected to pay them in gold, and silver was the dearer metal. Soon after came the fall of silver. It was sudden, unexpected, and unprecedented in amount. In 1877 the cry was raised that the act of 1873 had taken from the people the advantage of this depreciation for scaling down their public debt, and a bill was introduced for the coinage of silver dollars of 412½ grains, with unlimited legal tender, and containing instructions to the Secretary of the Treasury to pay them out in discharging the coin obligations of the government. It was at once protested that

this was practical repudiation. In 1873 silver was demonetized, and the public debt was *ipso facto* made payable in gold coin only. Subsequently millions of this debt changed hands and was bought from day to day by tens of thousands of "innocent third parties," who thereby acquired the right to ultimate payment in gold coin as then current. Millions of the debt were refunded, the old bonds being redeemed and new ones sold. The face of the new bonds disclosed a contract to pay coin, and the law recognized no coin except gold. All rational doubt as to whether "coin of the present standard value" meant gold which was in universal and exclusive use in all coin transactions, or silver, which was never used except for export, was put at rest by the formal abrogation of silver in the regular course of legislation. At this stage the "Bland Bill" intervenes, and orders the manufacture of millions of a new coin having a much less value than that which the bond specifies, and makes them legal tender for all debts, public and private. Is it possible for anybody to show that this is not a repudiation of a portion of the public debt?

As regards those bonds which were refunded after the passage of the Bland bill, there seems to be no doubt that they are honestly payable either in silver or in gold. It was a clear case of *caveat emptor*. The great notoriety given to the discussion of this measure was abundant notice to all purchasers that the bonds which they were buying for gold would be liable to redemption in silver. Still there is a moral taint about even these. The procedure was a "shoving" of a debased standard upon the valuation of a class of securities which every consideration of prudence, sound finance, and far-sighted economy should have induced Congress to keep up to the best possible standard. The bonds of the United States are chiefly used for those investments which ought to be the least liable to fluctuation in value, and the most sure of full payment without rebate at maturity. To speak more in detail, they are security for the payment of the notes of National Banks, which are in the pocket of every man who is able to buy his bread: they are the form which idle funds often take while waiting for the demands of trade: they are the best and most proper investment of trust funds and the savings of myriads of poor people to whom security is of more importance than high interest: they are the best "gilt-edged" security for loans by the deposit of which money can be borrowed at the lowest rate of interest. To debase the ultimate valuation of these bonds was to introduce the same vice into them which has heretofore clouded the name of American finance—paying debts in a bad currency and instability of value: and the infection was introduced just where it should have been most carefully fended off.

The views which were entertained by the opponents of the Bland bill as to its tendencies and ultimate effects agreed as to generalities though differing as to details. It was patent that unless the Government restricted the coinage and monopolized

the profits of it, the whole financial fabric of the country would within a year or so drop from a gold to a silver basis involving alike all public and private credits. Obligations contracted in gold or equivalent greenbacks would be compulsorily paid in silver, inflicting great damage and loss upon everybody, especially upon the poorer and less fortunate classes, and opening chances to shrewd speculators to realize handsome profits out of the disturbances in nominal values. The men of capital and the banks would be able to take care of themselves in the main, though sure to suffer some loss. It is their business to look out for such disturbances, and being duly warned, would be well advised as to the means of breaking the force of the blow, though not avoiding it altogether. But for poor people who earn daily wages, for the dependent classes in general, and for all people who do not know how or who cannot provide against such changes, there would be no escape. Fortunately the proposition was modified by limiting the coinage to not less than two millions nor more than four millions per month, and closing the mints to coinage on private account. This was a saving clause of great value. It had the effect of postponing the evil day, and it was hoped that before the egg would have time to hatch people would take a sober second thought and reverse the plan.

As the Bland bill finally passed, the effects of its provisions would require time to develop themselves in consequence of these limitations. Before it can have any serious effect two conditions must be fulfilled.

1st. A quantity of silver must be coined large enough to form an important factor in the total volume of currency.

2d. It must be forced into general circulation.

The quantity of silver required to make the measure effective is unknown. Much must depend upon the general state of trade and the demand for money, especially metallic money. At one period seventy-five or even fifty millions might make it of serious account, at another two hundred millions might be required. In general from a hundred to one hundred and fifty millions might be on the average sufficient to render it a potent factor.

In order that the Bland bill may become operative the silver must be forced into circulation. At present only a slight and inefficient pressure is exerted in that direction, and the circulation is merely nominal. Small amounts are paid out, but they come back by the shortest route to the treasury. The explanation is very instructive and curious. It is customary to state a certain law of money in the well-worn adage that "an inferior currency will if permitted expel a superior one from circulation." This is because persons prefer to pay their debts in the cheaper currency, provided they are perfectly free to do so. The statement presupposes that it is optional with the debtor which of the two he will pay, and not with the creditor which of the two he

will receive. Under that assumption the law is undoubtedly universal. But as the Bland-Allison law now stands the option is practically reversed. The creditor who presents a draft at the treasury is left to choose, in greatest part, whether he will receive silver or gold, and of course chooses gold. Probably this is the first time in the history of money where an attempt has been made to put an inferior currency into circulation without forcing the payee to accept it. Furthermore, the treasury receives back without limit the silver which it pays out in small sums. If the law seriously means to put silver into practical circulation it is evident that it must pay it out forcibly, and rigorously limit the conditions upon which it will receive it. Persons who exact payments from the treasury will take all the gold the treasury will give and leave the silver. They do so because they prefer gold (or equivalent greenbacks). They prefer it because silver is sixteen times heavier, and thirty-five times bulkier; because gold is worth quite as much in the form of bullion as in coin, while silver is not; because it will pay foreign debts at its nominal value, while silver will not, and because it is intrinsically more valuable than silver of like nominal amount. This may be all wrong, a mere prejudice, it may bespeak a want of appreciation, etc. etc., but there is no accounting for tastes in this world, and the preference is shared by ninety-nine men out of a hundred who use money. The use of paper certificates helps the matter somewhat, and the fact that they are receivable at the custom houses helps it a great deal, but the objection in part still remains. No man handles a silver certificate without thinking of the kind of coin in which it is redeemable, and he thinks also that he would prefer it if it were redeemable in gold. There is but one way to circulate them, and that is to force them out and keep them out

It is evident that the Bland bill does not meet the purpose for which it is designed, and if is to become operative some additional legislation is necessary. Members of Congress are beginning to perceive this. They attribute the failure to produce the expected results to sundry causes, one being the discrimination made by the banks against silver which they regard as contumacious and actuated by a Wall Street prejudice.

But surely a little reflection ought to show the gentlemen who make this charge, that the prejudices of the banks and bankers have no more to do with their action in this matter than their religious or political convictions. It is the business of banks to advance money, and the particular kind of money which customers want. Customers prefer greenbacks to silver certificates. If they cannot get just what they want from the National banks they will get it somewhere else, provided they can do so on more satisfactory terms. It is not the banks who primarily give effect to the discrimination, but people who receive money from the banks. Any attempt to compel or persuade the banks to do otherwise, would

be about as politic and rational as to attempt to persuade or compel a commission merchant to mix turnips and potatoes and sell the whole as potatoes. It was the Government which originated and sustains the discrimination, and not the banks. It forcibly issued paper redeemable in silver dollars only; it issued also paper redeemable in gold dollars; the people *feel* if they do not see the difference; they act accordingly, and the banks are compelled to follow.

To make the Bland bill operative, then, it is necessary for the Government to take decisive measures to force silver into general circulation. But when such decisive measures are taken, certain consequences must follow, which we must endeavor to forecast. These measures must obviously restrict the right of payees except the Treasury to demand gold at face value, and sustain the right of payers to tender silver to everybody except to the government. But the instant any such action is taken the consequences are obvious—gold will begin to command a premium, and the moment it commands a premium, however small, it ceases to be a standard or common denominator of value. With the lapse of time the premium of gold will thereafter increase, until it has become about equal to the difference between the bullion values of gold and silver in the markets. Thus the enforcement of the circulation of silver will gradually reduce the financial fabric of the country to the standard of the silver dollar.

But whether the silver is speedily forced into circulation or not, the continued operation of the Bland bill as now administered will have the tendency to bring our money down to the silver denominator in the course of a few years. True, it may fail to do so and we may escape, but there is danger of it sufficiently serious to demand earnest attention. At present there is a golden tide setting from Europe into this country. The bad crops, the depression of industry in Europe, and its revival in America with splendid harvests, have caused a heavy balance of trade in our favor. That balance is paid in gold because there is a demand for it here and because Europe has nothing better to send. But a favorable balance of trade is only an ephemeral state of affairs. Just as surely as the pendulum now swinging to the right will in a little time swing back to the left, just so surely will the balance of trade turn in the opposite direction and the golden stream will flow back to Europe. In the mean time the Treasury is rapidly piling up useless silver in its vaults. Every silver dollar put into its strong box represents an equal bullion value of gold or greenbacks taken out of it. When the balance of trade turns, gold will be wanted for export. No silver, be it observed, but only gold. Debts are not paid with silver in Europe. Greenbacks and gold certificates will then be presented at the Treasury (or bank notes at the banks, which in the end amounts to the same thing), for redemption in coin; and gold will be demanded. If the Treasury insists on paying greenbacks in silver, the crisis

will instantly be precipitated—gold will command a premium over silver coin, and the premium will increase as gold becomes scarcer. If the Treasury pays gold freely, how long will it be able to do so, and will it be able to meet the drain until the tide turns again? It will then be burning its gold candle at both ends, paying out gold for export and at the same time steadily converting its metallic reserves into silver. If it had never embarked upon this silver escapade, it would have in its vaults an additional amount of gold or greenbacks equal to the silver it will have purchased and have an abundant strength to meet the heaviest stress which could possibly be put upon its gold balance. But under the continued operation of the present law there will be a powerful temptation and stress—perhaps an irresistible one—to restrict gold payments and roll out the hoard of silver coin. With a possible change of administration with changed financial views, the policy of paying out silver more forcibly and restricting gold payments to small amounts may be adopted at any time. A Treasury which is piling up silver at the rate of three millions a month and continuing it without limit and unable or unwilling to force it into currency, is necessarily shifting the centre of gravity as between gold and silver over towards the silver side and must be tending towards a silver basis.

The ultimate result then of the Bland bill is to bring the standard of money (or the common denominator of value, as Prof. Walker would say) from its present gold value down to the bullion value of the silver dollar. It is only a question of time, and no very long time at that. The effect of such a change will be ultimately the same as if a law had been passed debasing the gold coinage an equal amount—putting, say, one-eighth less gold in every coin struck at the mint, and making the debased coins legal tender at their old nominal value. If the proposition had been made to debase the gold coinage outright from 12 to 15 per cent., probably those who voted for the Bland bill would have shrunk from such an ignominious procedure. And yet how would it have differed from the law actually passed? Morally and economically in no essential respect whatever. Instead of striking the debased coin in gold they struck it in silver, and that is the whole difference. The debased gold coins would have been preferable, for they could have been instantly readjusted to foreign and domestic trade and retained all the advantages of a gold currency, while silver is open to objections purely as silver chiefly arising from the want of adjustment and harmony with the monetary movements in the nations with which we trade.

The advocates of the Bland bill frequently enlarged upon the advantage and necessity of a bimetallic standard. They asserted the probable insufficiency of gold alone to sustain specie payments and the necessity of calling in silver to co-operate. The quantity of gold required by the gold-using nations especially at the present time, they said, was enormous, and they pointed at

the comparatively limited supply. While the nations of Europe are shifting to a gold standard, if we also shift from paper to gold and undertake to redeem our vast volume of paper currency in that metal, the demand for it will be so great that either we shall break down in attempting to resume, or if we succeed, shall run up the price of gold so high that debtors will be ruined and the existing depression in industry become still more disastrous. Hence we must call silver to our aid and adopt a bimetallic standard. We shall then have the total body of two great stocks of precious metals in which to redeem instead of one.

Before proceeding to discuss this argument it is necessary to form some conception as to what they meant by the term bimetallic standard.

The sense in which political economists employ the term bimetallic currency, is that of a currency in which the coinage of gold and silver is free, and both metals are unlimited legal tender. It happens that there is no problem in the use of money with which the nations of the world have more experience and more positive knowledge than the attempt to use a bimetallic currency in this acceptation of the term. The experience has ranged through centuries, and up to the most recent times: it is found among the annals of all civilized nations, and the examples are exceedingly numerous. The results have been uniformly failures. Not a solitary exception has ever been known. The reason is obvious and the explanation perfect. When the law provides for the coinage and legal tender it fixes at the same time the relative money values of the two metals, *i. e.*, declares how much silver shall be equivalent to a given quantity of gold. It begins by fixing these relative values at the same proportion as is found to rule in the markets at the time. But the relative market value of the two metals is continually changing, while the mint value cannot be changed without recoinage. And as soon as a persistent change is developed between the mint ratio and the market ratio, it becomes profitable to melt down or export one of the coined metals which inevitably disappears from circulation while the other metal remains and alone performs the function of money. The currency then ceases to be bimetallic and becomes monometallic. In the fluctuations of the market prices, the ratio which had formerly been higher than the mint ratio has become lower. The result was that the metal which at first disappeared came back, and the metal which at first remained disappeared in turn. Considered with reference to long periods of years the intended bimetallic currency becomes what is termed an alternate currency; that is, a currency in which there is a continuous succession of famines of each metal alternately.

No rational person, however, has ever doubted the necessity of using both gold and silver as money. The question which

has arisen is, under what laws and conditions is it possible to secure the proper use of both? Late in the eighteenth century financiers and statesmen had learned that such use could not be secure under existing laws, and proceeded to inquire what uses it was possible to secure and what laws would effectuate such uses. England was the first country to put in operation a practical and successful solution of the problem. Her statesmen recognized that gold and silver would not circulate together so long as the coinage of both metals was free, and both were unlimited in volume and legal tender. She therefore closed her mints in 1816 to the coinage of silver, leaving the status of gold unchanged. Silver was coined by the government on its own account, its volume limited to what was deemed necessary for purposes of trade, and its legal tender limited to forty shillings. The plan has worked to her perfect satisfaction ever since, and her currency is universally admitted to be safe, secure, and convenient. She has all the gold and silver she needs, which is the utmost that can be said in favor of any currency.

The reasons for choosing gold for free coinage were chiefly two. In the first place, it is important that the money unit should maintain as nearly as practicable a constant value in comparison with other commodities, taken as a whole. This property belongs to gold in a higher degree than to any other available money substance, and is therefore preferable for furnishing the common denominator of value. In the second place, the aggregate of large payments require more money than the aggregate of small ones. Gold is much more convenient and economical for large payments than silver, and is therefore the best substance for the principal bulk of a currency. Silver is utilized to the most advantage when used as the subsidiary of gold. It should be its servant and not its master.

In order, then, to keep the two metals in constant use, each performing its own function, it is necessary to lay restrictions upon the coinage of silver, while gold may be coined without limit and freely. The quantity of silver must be limited and the government must monopolize the right to send it to the mint. It must also be debased in coining it, to prevent it from flying out of currency whenever the market price of silver is very high. The question now arises, What quantity of silver should be put into circulation? should it be very large—say equal to the amount of gold—or very much less? The question which has concerned the American bimetalists seems to be, What is the utmost amount of silver which the country can be forced to absorb? In reality the evident true policy should be to ascertain what is the smallest amount which can be used without inconvenience; and the same is true of gold. Money is for the purpose of facilitating exchanges. A mechanic who knows that oil is good and useful for facilitating the movements of machinery, would never undertake to see how much oil he could use for lubrication, but how little.

In the search for an illustration of the quantity of silver which a country can absorb, the bimetalists have fixed upon France. Never was a case more grossly misunderstood and misrepresented.

The function which silver now performs in France may be made intelligible by the following illustration: Suppose all our $1, $2, $5, and $10 notes were not in existence, and that the same amount of one dollar notes were circulating in their place. Suppose, further, that the promise to pay one gold dollar on demand, instead of being printed on a piece of paper, were stamped upon a circular piece of silver worth about 81 or 82 cents. In all essential respects such a currency would correspond to French silver. The latter is a token currency, circulating at 18 or 20 per cent. above its real value because it is convertible into gold. The amount, though large, is still limited by stopping the coinage, and the quantity in actual circulation is rapidly diminishing, being gathered into the Bank of France in an enormous hoard. The French government, however, is firmly resolved on maintaining the gold standard, and the only important difference between her silver and a redeemable paper currency is that the former can never depreciate below its bullion value. In no sense can France be said at present to be employing the bimetallic standard. Her standard is gold as truly as that of England or Germany. Her silver is the heritage bequeathed to her from antiquity, and is now a burden rather than a source of wealth. The present outlook is that before many years France will become a gold using nation exclusively, as she is already in chief part. If it were possible to demonetize silver at once without serious loss and doing violence to the peculiar habits of her poorer people, there is little doubt that it would be done.

The Bland bill provides for a silver currency radically different from that of France. It is calculated to push the country into a situation involving not only the difficulties and evils which France would most gladly escape from if she could, but others far more serious. The growing spectre of the silver dollar has no limit fixed to the size which it may reach. No gross sum is named at which the coinage must stop. These dollars are not convertible into gold at face value, as the French five-franc pieces are. If it were not that the volume now out is small, and were they not receivable in taxes and duties in place of so much gold, they would be at a discount or gold at a premium.

The question how much silver *can* our country absorb is one thing, and the question how much *ought* it to be made to absorb is another. The first question can be finally answered only after trial; the second one has been answered already. It ought to be made to absorb just as little as it can get along with without inconvenience. That it might use a considerable number of silver dollars of the present standard seems probable. They could perform the function which is now performed by the $1 and $2 notes, but somehow the people seem to prefer the notes

as a rule, though the coins show a tendency to circulate moderately. It is not probable, however, that more than 40,000,000 of them could be kept in voluntary circulation, and that number seems very large.

The common belief which seems to pervade the minds of the advocates of the present silver policy is, that in the first place we need an enormous amount of metallic money in this country, and in the second place that the gold supply is insufficient to meet alone this demand for metallic money without increasing greatly its price and correspondingly disturbing all values.

The natural laws governing the use of money seem at first to be highly paradoxical. It is a well known fact in the history of money, that the more abundant the supply the greater is the complaint of scarcity. This complaint has risen again and again, and has too frequently resulted in the application of what seemed at first the normal remedy—an issue of more money by the Government—but which proved to be a source of aggravation, making money scarcer than ever and in greater demand. It was so in the old colonial times, and it has been so in this country ever since. The same experience has been repeatedly observed in Europe, where the difficulty is well understood and always treated with the appropriate remedy whenever it shows a tendency to manifest itself, which is very seldom. The explanation is not difficult. A little examination shows that the complaint has always arisen in poorer countries and regions or States less advanced in material wealth and improvements, and the complaint has been directed against richer neighbors, who have invariably been accused of prosecuting a policy designed to monopolize money at the expense of poorer people. In our own country there is and always has been unquestionably a dearth of money in the West and South and a plethora in the Eastern and Middle States. The want of more money in the West and South is probably a real want. The cause is that money goes where goods are for sale and leaves a country where nothing is to be bought. Once a year for a space of about two months the great harvests of the West and the cotton crop of the South are ready for the market, and forthwith the westward and southward bound trains are laden with money from the Eastern and Middle State banks, and it is scattered broadcast over a vast expanse of country. But within a month it is back in the vaults it came from, and for ten long dreary months the people of those regions are impecunious again. The money realized for a year's farming has gone to pay debts or the annual interest on mortgages and to buy a year's outfit of the necessaries of life. In short, money goes where there are commodities to be bought. The fancy that the ten months' scarcity can be relieved by increasing the total volume of currency is utterly absurd. What have the impecunious classes to give in exchange for it? or what would they have to sell which they have not already? and how are they to obtain

their share of the increase? Mr. Weaver's proposition to make every tenth or twelfth citizen a creditor of the Government by Act of Congress, and to issue $500,000,000 of money to pay this trumped-up indebtedness, would be one way of distributing money, but how long would it stay in the hands of impecunious people? If it had any appreciable value after such a watering, it would concentrate into the great reservoirs even more voluminously than at present, and in a month the people who had received it would be worse off for money and everything which money can buy than they are at present. What the people of those States want is capital with organized and diversified industry, and they have confounded the want of capital and production with the want of money. If they have the former, money will come fast enough; and if they have them not, money will not come to them of its own momentum, nor will it abide with them even though Congress were to shower down gold upon them.

It is the general opinion of financiers that our present currency is much in excess of our needs, and that the plethora is very injurious in its effects upon trade, the business morals, and material growth of the country. But whether it be so or not, it would be immaterial to inquire so long as we are a monometallic gold-using nation, for under such a regime the quantity of money in the country would regulate itself far better than any human device could do—for when money became scarce it would flow in from abroad, and when it became redundant would go to other countries. But if we are to be a monometallic silver-using nation it is otherwise. We are liable at one time to a glut of silver money and at another to a scarcity. Since the nations with whom we trade use gold, they would not receive our excess of silver nor send us theirs at the precise time when such movements would be most opportune. The self-regulating quality would be wanting.

The notion that there is not gold enough in the world is easily answered. As Mr. David A. Wells remarked: "If a single silver three-cent piece were capable of sufficient divisibility or rapidity of circulation, it would suffice to effect all the monetary exchanges of the whole civilized world." This is but an extreme illustration of a general truth, which ought to be apparent upon the slightest reflection. It is a perfectly safe assertion that if the quantity of gold in the world and the annual supply of it were one-tenth what they are at present, there would still be enough of it for all the monetary purposes of the world, and it would serve those purposes pretty nearly as well as it does at present; the only notable difference would be that the sovereigns, five-dollar pieces, and twenty-franc pieces would be inconveniently small in size, while pieces of larger value would be even more convenient than now. And if the quantity of gold were ten times as great as at present, it would be no better for monetary purposes. Why! here is silver, of which the quantity now in coin is estimated to

be twelve or fifteen times more than that of gold when measured in pounds, and twenty-five or thirty times more when measured in cubic inches, and yet the purchasing power of the whole is actually less! As a monetary medium it is less capable of effecting exchanges than the world's supply of gold. In short, the quantity now in the world either of gold or silver has nothing to do with the question. "Whatever there is, is enough."

To the assertion that the adoption of the monometallic gold standard by the United States will increase the demand for, and consequent price of gold and depreciate the prices of commodities, the reply is that we have practically the gold standard already, and very nearly enough metal in the country to maintain gold payments. During the last three years the influx has been constant and largely at the expense of the nations of Europe. Yet neither has it risen in price nor have commodities depreciated. On the contrary, prices have risen with a rapidity and to a degree of which our commercial history furnishes few parallels. If the fifty million silver dollars now buried in the Treasury were converted into gold, specie payment would be as sure and safe as it is with the Bank of England. Nor is there reason to fear that the acquisition of a much larger sum would materially affect the price of gold or its purchasing power. Prices have been affected but little by the acquisitions of France and Germany, which have been much greater in amount than we should have occasion for. It is true that in the last ten years prices have fluctuated greatly, but it is clear that the fluctuations have been almost independent of changes in the precious metals. They fell in 1873 to 1878 because a time of reckoning, long deferred, had at last come, and people were more anxious to pay and collect debts than they were to buy and contract new ones. Prices rose in 1879 because debts had been liquidated or sponged out in the courts; people had lived close, were ready to go to work and to trade with clean slates, and had money and credit enough to begin buying again. The effect of the rise and fall of the precious metals was wholly inappreciable—as much so as the rise and fall of the tides upon the billows of mid-ocean.

The effect of the Bland bill may be thus summarized:—

1. At present it is inoperative, because the silver is not forced into circulation.

2. It cannot be forced into circulation without being received at a discount or forcing gold up to a premium and driving it out of the country.

3. Its present effect is limited to a steady and gradual accumulation of silver in the treasury in the place of gold.

4. The growth of this silver hoard is regular and constant, and only requires time to become vast in amount. At any time in the near future a turn in the balance of trade must bring down upon the treasury a demand for gold to export. If the treasury pays gold its stock will be soon exhausted, and it will have no-

thing but silver to redeem its notes with. If it restricts gold payments the result will be the same—gold will command a premium, and cease to be a standard of value or to take part in the circulation of the country.

5. In general, the effect of the Bland bill is in the end to expel gold from the country, and bring the money standard down to that of the debased silver dollar; in short, to make us a monometallic silver-using nation.

We revert, then, to the inquiry with which we started this discussion. Is the present silver law "well enough" to require that it be let alone? It is not well enough, and the time to amend it is the present. Within a year it is probable that the first crop will be reaped from our neglect to do so. Several hundred millions of six per cents are to be refunded. Perhaps some "bloated bondholders" will be insolent enough to demand an understanding before they buy new bonds, whether they are to be paid principal and interest in gold coin of the present standard or in the debased silver dollars. And when the attention of buyers and syndicates is fairly called to the predicament, they must realize the principle of *caveat emptor*.

Upon this communication Mr. DOOLITTLE remarked that he believed there was much force in the position taken by the U. S. Commissioners at the late International Monetary Conference, that a combined and general *consensus* of the great monetary powers of the world would be able to sustain a constant ratio in the values of the two metals, and that it seemed to be a wise policy to promote such action in order to have the entire volumes of the two metals combined as the basis of values, which would be far more stable than if it consisted of one metal only.

Mr. E. B. ELLIOTT remarked that the view so confidently entertained and proclaimed by certain writers of prominence, both in this country and Europe, that France, by virtue of its fixed legal ratio of 15½ to 1, can control, and for many years has controlled and determined the current relative value of gold and silver in the world's market, is not in accord with fact, and that the conclusions based on it are groundless.

It was moved that the discussion of this subject be resumed at the next meeting, which motion was carried.

The meeting then adjourned.

175TH MEETING. FEBRUARY 14, 1880.

Vice-President TAYLOR in the Chair.

Forty-eight members present.

The Chair announced to the Society the election to membership of Mr. JOHN HENRY COMSTOCK and Mr. EBEN JENKS LOOMIS.

The order of exercises, which had been fixed for the evening according to the terms of adjournment of the preceding meeting, was a continuation of the discussion of the Silver Question, but priority was given to a communication by Mr. C. A. WHITE on

THE SUBJECT OF THE PERMIAN FORMATION IN NORTH AMERICA.

Dr. White began by remarking upon the practice of giving to geological formations names derived from localities where they had first been found to have peculiar development and importance, like the Silurian and Devonian. In this way the Carboniferous system had derived its name from the fact that in Europe, where it was most studied at an early stage of geological science, it contained coal and carbonaceous shales. The progress of investigation, however, subsequently led to the conviction that really less coal is found in the Carboniferous than in other rocks. In the western portion of the United States, the Carboniferous formations are, as a rule, quite destitute of coal.

Those rocks which are assigned to the Carboniferous in the West have, in general, a fauna agreeing with Carboniferous fossils of Europe and eastern America; but it appears that, in some instances, the fauna found in the lower part of the series in one locality, have their correlations in the upper part of the series in another locality. This is explicable upon the assumption that the physical condition necessary for the development of a faunal group may have prevailed early in one locality and late in another.

Having in view these considerations, Dr. White then indicated the three principal subdivisions of the Carboniferous system in Europe, viz.:—

1. Permian sandstones and shales;
2. Coal measures, sandstone, limestone, and shales;
3. Lower Carboniferous limestone without coal;

and adverted to the fact that the two lower divisions are found

in America with the same general lithological characters and many identical species in the respective members. But the Permian had not been so satisfactorily established, in America. In Europe the Permian is often wanting from its proper place, and its universality there has been regarded as generally doubtful. Until 1858 it was not supposed to exist, or at least it was not pretended to have been discovered in America. In 1858 its existence was announced almost simultaneously in Texas and in Kansas. A little later Prof. I. O. White announced Permian plants from West Virginia. These announcements were at first somewhat eagerly contested, but the discussion fell to the ground as profitless, with the general impression that the Permian had not been established. The palæontological difficulties arise from the fact already noted, that Russian Permian types are found in the West in the middle and lower Carboniferous; and types which are low in the series in Europe are found high in the series in America, while many types are common to all parts of the system. There are, however, certain forms which have always been regarded as distinctly Permian.

Recently Mr. Walcott has brought from southern Utah or northern Arizona a series of fossils obtained from beds overlying the Aubrey limestone (=middle Carboniferous or Coal measures). The beds from which they came are locally named the Shinarump, and have been doubtfully assigned to the Lower Trias, with a reservation in favor of the possibility that they might be Permian. These fossils are palæozoic, with the exception of one or two forms which may be mesozoic. Among them is a species of Bakewellia, which has never been found above the Permian; and the group, as a whole, seems to indicate that the beds in question belong decidedly to that age. Although this discovery would make these beds and their equivalents throughout the West the correlatives of the Permian of Europe, it does not follow that the periods were strictly coëval in the two continents.

Mr. GILBERT remarked upon the relations of these Permian beds to the Aubrey limestone upon which they rest, and stated that the contact was frequently, and perhaps generally, unconformable in the vicinity of the locality where these fossils were found. But no such unconformity had been detected in higher horizons, and hence no physical break had enabled the separation

of these Permian beds from the Trias above, though such a separation was easily made from the strata below.

Mr. POWELL remarked that the continuity of these strata with strata overlying the local Carboniferous of the Uinta Mountains had been traced out. Mr. KING had also traced the continuity of the same beds from the Great Basin through the Wasatch to the slopes of the Uinta Mountains, connecting the horizon with that ascertained by Mr. Powell. Thus, the equivalence of the widely separated exposures of the Great Basin, the Uintas and Arizona rested upon stratigraphic evidence, independently of palæontological, and the palæontological evidence now confirmed the conclusions originally based upon the strata themselves, for the fossils found by Mr. King were substantially the same as those found by Mr. Walcott.

Mr. E. J. FARQUHAR read a paper

ON A REMARKABLE HAIL-STORM WHICH PASSED OVER MONTGOMERY COUNTY, MARYLAND, APRIL 28, 1878.

After remarks on this paper by several members, the brief time remaining was occupied by Mr. DOOLITTLE in comments upon some subsidiary points in the paper of Mr. DUTTON, of the preceding meeting, and by brief remarks by other members of the Society.

A general disposition having been manifested to resume the discussion of the Silver Question, with more time to devote to argument, it was decided to resume the discussion of it at the next meeting.

The Society then adjourned.

176TH MEETING. FEBRUARY 28, 1880.

The President in the Chair.

Fifty members present.

The minutes of the last meeting were read and adopted.

Pursuant to the resolution adopted at the last meeting, the order of proceedings for the evening consisted in a further discussion of the Silver Question. Prior thereto, Mr. Juvet was

invited by the General Committee to exhibit to the Society a Time Globe. This consisted of a terrestrial globe, mounted upon a stand, and containing within it a driving clock. The winding stem of the clock-work projected at the south pole, in the form of the feather of an arrow; and near the south pole was an attachment for accelerating or retarding the movement, so as to adjust its rotation to the true time. The axis being set at the same inclination to the local horizon as the axis of the earth, the globe is in position. The local time and the time of any other point on the globe is at once read by finding the intersection of its meridian with the fixed equatorial circle encircling the globe. Upon this circle the twenty-four hours and minutes are graduated in inverse order.

The discussion of the Silver Question was then resumed.

Remarks of MR. E. B. ELLIOTT.

Mr. Elliott opposed the view advocated by "bi-metallists," so-called, that the legal ratio of France alone, or the legal ratios established by the several commercial countries of the world, whether independently or in harmony, could govern and fix the market ratio of gold to silver.

The market ratio was governed by different considerations than merely legal enactments, granting to any person making payment of a debt, the option to make that payment either in gold or in silver, at a *fixed* ratio of valuation. The market ratio is not permanently or appreciably influenced by such legal ratios. If the fixed legal ratio does not conform to the current market ratio, payments, in whatever metal or commodity made, would be made on the basis of the relatively over-valued metal as the standard money of account and payment.

If payments be actually made in the undervalued metal, a premium will be claimed and assented to equal to the difference between the legal and the market rates of valuation of the two metals. The establishment of a legal ratio, in any country, does not of necessity determine the metal in which payments shall actually be made; it only determines the metal with reference to which accounts shall be kept and payments made. If silver be the relatively over-valued metal, the unit of account (the dollar for instance) will be referred to the silver unit as the standard (instead of the gold unit), and *vice versa;* but payments may be,

and doubtless extensively will be, made in gold, valued at the market premium over silver from time to time prevailing.

He read from a publication by Mr. Cernuschi, the well-known and bold advocate of "bi-metallism," so called, claiming that, if the leading commercial countries of the world can be induced to unite in the establishment, by statute law, of any fixed ratio of value, however extreme, between the two precious metals (gold and silver) as the basis of payment, the metal selected to be always at the option of the person or party making the payment, whether this ratio be 10 to 1, 15½ to 1, 20 to 1, or in the extreme case of 1 to 1, the market ratio must of necessity conform to this conventional ratio; and, in the extreme case mentioned, an ounce of gold becomes the equal in market value of an ounce of silver; and, further, taking the ground that, if either of the two metals should be demonetized—that is, should cease to be a legal tender—such metal should become valueless. If "international legislation" is substituted for the existing systems, "on it alone," he says, "will depend the relative value of gold and silver. If the international legislation is mono-metallic, the metal which is not money, will lose so much of its value as to be no longer precious. If the international legislation be bi-metallic, the relative value of gold and silver which it recognizes will remain always and everywhere invariable."

Mr. Elliott, while duly appreciating the boldness of the writer in following his principles to their legitimate and logical conclusions, dissented from his views.

It was his opinion that, after the proposed enactment of the fixed ratio by all commercial nations, every debtor will choose to pay his debt of *one thousand* dollars (for example) by the delivery of *one thousand* ounces of silver, rather than by that of *one thousand* ounces of gold. Silver would rule as the *standard* of account and of payment; and payments, when made in gold, would be made at a premium determined by commerce upon the relatively cheaper silver standard.

The relative market value of the two metals, like the market values of other commodities, will not conform to any fixed ratio established by the breath of legislators, but will ever fluctuate, obediently regardful of the ordinary laws of trade, involving the familiar elements of supply, demand, and cost of reproduction, as controlling factors.

Remarks of Mr. H. N. BURCHARD.

Mr. Burchard, in the course of his remarks, first defended the action of the government in re-establishing the coinage of the standard silver dollar of $412\frac{1}{2}$ grains; and, secondly, discussed bi-metalism, in respect to the relation between the production of the precious metals, and their relative use as money by the principal nations of the world.

He stated that the coinage of the standard silver dollar was no violation of the letter or spirit of the refunding law of 1870, under which bonds to the amount of $1,492,264,850 have been issued. That act made all the bonds issued under its authority "payable in coin of the present (1870) standard value." The coin of that standard was a silver dollar unit of value, of $371\frac{1}{6}$ grains of pure silver as well as gold coins of $23\frac{22}{100}$ grains of pure gold to the dollar. It is a well-settled principle of law and equity, that all persons contracting with government or municipal authorities are bound by the terms under which the contract is made, and their rights are controlled by its provisions. The purchasers of all the refunding bonds issued under the act of 1870 obtained obligations payable at the option of the United States, at their maturity, in silver or gold dollars. The Secretary of the Treasury, in a letter to Hon. Hugh McCulloch, under date of March 20, 1878, says: "I doubt if the right of the government to pay the bonds issued under the Refunding Act in the coin in legal existence at the date of the act can be questioned or defeated," and that "the law fixes the terms of the bonds, and not the department or its agents."

Even if at the time of the sale of the bonds silver dollars were not coined, it did not debar the United States from thereafter coining silver dollars, and with them paying the bonds under the contract; and if for reasons of public policy it was deemed desirable to restore the so-called double standard, and coin the silver dollars, no legal, equitable, or moral reason, Mr. Burchard insisted, could be advanced for excepting these bonds from payment pursuant to the terms of the contract. Such had been the deliberate judgment of Congress, which, early in 1878, by a vote of 42 to 20 in the Senate, and 189 to 79 in the House, passed Senator Matthew's resolution, affirming the legality and honesty of the coinage and use of the silver dollar in the payment of these bonds. This action was followed by the passage of the act for

the coinage of the standard silver dollar, on the 28th of February, 1878, under which $54,806,050 in silver dollars have been coined. This in no way appears to have injured the credit of the government or of the bonds; the 4½ per cent. bonds, then scarcely above par, are now at a premium of eight per cent., while the 4 per cent. bonds are at a premium that yields but 3$\frac{7}{16}$ per cent. to the investor.

To show that the act for the coinage of silver dollars was neither dishonest nor inequitable, Mr. Burchard stated that when it was passed in 1878, the purchasing power of silver in this country—measured by the comparative prices of commodities—was the same as in 1870, when the refunding act was passed. He stated that a careful examination of the average yearly values of commodities for the last ten years had been made in the Mint Bureau, and the results were to be found in the last annual report of the director. The prices ascertained by dividing values by quantities of eighty leading articles of export, embracing 85 per cent. of the total value of the exports of the United States, showed an increase from 1870 to 1879, in the purchasing power of United States notes of 47 per cent., and in gold of 14 per cent., and of the latter, since 1873, of over 20 per cent., while the purchasing power of silver in 1878 was at 100, compared with itself as it stood in 1870.

To show that the amount of the relative quantity or annual production of gold and silver did not determine their relative market values, the speaker exhibited upon a chart the annual production of the precious metals in successive decades, from 1700 to 1870, and the average market price at Hamburgh for the same periods. He pointed out the rise in the market value of silver up to near the close of the 18th century, and that when silver was being produced relatively to gold in quantities three times greater than in the preceding half century, the market price of silver was advancing instead of declining, and that when gold and silver production had become equal by the decline in the amount of silver produced, the market value of silver, compared with gold, had declined instead of advancing.

The only apparent conformity of the law, as claimed, of a higher relative price for either metal on account of relative diminished production, the speaker stated to be from 1850 to 1870, but which he said resulted from legislation which gave an exclusive

use of silver in the Orient. In proof of that, he argued the legislation of different countries had made, and to a large degree controlled the market rate.

Mr. Burchard showed the legal valuations, from time to time, of different countries, some of which, as he said, were above, and some of them at the same time below the market rate; and he insisted that the facts in the financial history of the past century seemed to show to him most conclusively that the relative market rates of silver and gold had been little affected by the cost or amount of production, but by that legislation which created—sometimes in one and sometimes in another country—a greater use of one or the other of the precious metals, and so a greater or less demand for each.

He further said that the market rate at any place like Hamburgh would also be affected, not only by the legal exchangeable value of the two metals established by the country producing either, but also by the cost of transportation, time, and risk, in sending to the country whose legislation had given a greater use, and consequent demand, for one of the metals; that the changing balances of trade between countries giving legal preference to different metals, would affect the demand for each, and therefore affect the market value, and make fluctuations that, by the adoption of the same legal rate by all the commercial nations, would disappear.

In further proof that legislation had been the most potent factor in the past affecting relative value, he pointed out, as shown by his diagram, that when France, in 1785, adopted the ratio of $15\frac{1}{2}$ to 1, the ratio of nearly all the other countries of Europe was below 15 to 1, and in England 15.2 to 1; and that after the legislation of France, the relative market value of silver to that of gold declined to $15\frac{1}{2}$ of silver to 1 of gold; and that when England made operative her law, which made gold the only legal standard coin, by resuming gold payments, and by reason of her resumption and gold standard legislation, commencing in 1819, and during the next thirty years drew more gold from other countries than all the world produced in the period, the market prices of gold and silver became affected thereby, gold advancing in value, compared with silver.

The better price for silver after 1850, and to 1870, Mr. Burchard said resulted from the legislation of India, where silver, in

1835, had been made, and thereafter was the standard and currency. The increased production of gold augmented the prices of commodities in America and Europe, the balance of trade set in largely in favor of Asiatic countries, and silver was in demand, and was sent where gold had no legal status, and silver, therefore, as a consequence of the legislation of such countries, became comparatively more valuable, and commanded a higher market price.

Mr. Burchard therefore claimed that it seemed capable of historical demonstration that the fluctuation of the past in the relative market values of gold and silver had resulted from an increased or diminished use, occasioned chiefly by legislation. He said further that, after much consideration of these facts, the conclusion seemed to him irrefutable that the consent of all nations, embodied in the form of law, to the use of gold and silver as legal money, at fixed rates of value, would reduce the fluctuations in the relative values of the two metals to a minimum too insignificant to be regarded; and, in the language of the Secretary to one of our commissioners to the International Monetary Convention (Mr. Groesbeck), "that if it were possible for the leading commercial nations to fix by agreement an arbitrary relation between silver and gold, even though the market value might vary somewhat from time to time, it would be a measure of the greatest good to all nations."

Mr. A. J. WARNER, of the House of Representatives, was invited to participate in the discussion; and he remarked upon the relations of the quantity of gold in use to the price it held when measured by other commodities. He expressed the conviction that the acquisition of the necessary amount of gold which would be required to make us a thoroughly solvent, specie-paying nation upon a mono-metallic gold basis, would so raise the standard of value as to entail serious injustice.

Mr. M. H. DOOLITTLE called attention to a recent article in the *Atlantic Monthly*, which set forth that the future prospects of the production of the precious metals were indicative of a diminishing supply of gold, and a very steady supply of silver.

The Society then adjourned.

177TH MEETING. MARCH 13, 1880.

The President in the Chair.

Forty-five members present.

The minutes of the last meeting were read and approved.

The communication for the evening was by Mr. G. K. GILBERT,

ON THE OSCILLATIONS OF LAKE BONNEVILLE.

The paper was reserved by the author for publication.

Remarks upon this paper were made by Messrs. POWELL and DUTTON and the Society then adjourned.

178TH MEETING. MARCH 27, 1880.

The President in the Chair.

Thirty-nine members present.

Mr. E. B. ELLIOTT made a communication upon

THE CONSTRUCTION OF THE GOVERNMENT SINKING FUND,

especially illustrating the difference between the ordinary form of a sinking fund, and the form adopted for the reduction of the public debt of the United States.

[Abstract.]

1. A sinking fund is a fund for the reduction or extinguishment of a public debt. In the case of a sinking fund as ordinarily established, a fixed or constant sum together with interest on a fund already accumulated, or supposed to be accumulated, is the annual or periodical contribution to the fund. In the case of the United States, however, the sum just mentioned as fixed or constant, is not constant, but is proportioned to the current amount of the debt, an amount usually diminishing. In the case of the United States sinking fund the periodical contribution to the fund is required by law to be applied at once to the purchase and cancelling of a portion of the out-standing indebtedness bonds; so that the debt so far as affected by the sinking fund is a constantly diminishing quantity.

The annual or periodical contribution to the United States sinking fund is required by law to be one per cent. of the out-

standing indebtedness together with the interest on the fund supposed to be already accumulated.

2. Had the annual contribution to the sinking fund been a fixed sum in addition to the interest on the fund, these annual contributions would have formed a series in geometrical progression of which the common ratio would be the amount of one dollar for one year at the rate of interest realized on the investment of the funds; but since the contribution instead of being a fixed quantity is one per cent. of a diminishing quantity in addition to the interest on the fund, the entire annual contribution will constitute a geometrical progression, in which the common ratio is the amount of one dollar at a rate of interest one per cent. less than the rate at which investments can be made. For instance, if investments can be made at the rate of five per cent. per annum the entire annual contributions will progress at the rate of four per cent.

3. The language of the law is as follows:

SEC. 3694. The coin paid for duties on imported goods shall be set apart as a special fund, and shall be applied as follows:

First. To the payment in coin of the interest on the bonds and notes of the United States.

Second. To the purchase or payment of one per centum of the entire debt of the United States, to be made within each fiscal year, which is to be set apart as a sinking fund, and the interest of which shall, in like manner, be applied to the purchase or payment of the public debt, as the Secretary of the Treasury shall from time to time direct.

SEC. 3695. All bonds applied to the sinking fund, and all other United States bonds redeemed or paid by the United States, shall be cancelled and destroyed. A detailed record of the bonds so cancelled and destroyed shall be first made in the books of the Treasury Department. The amount of the bond of each class that have been cancelled and destroyed shall be deducted respectively from the amount of each class of the outstanding debt of the United States.

SEC. 3696. In addition to other amounts that may be applied to the redemption or payment of the public debt, an amount equal to the interest on all bonds belonging to the sinking fund, shall be applied, as the Secretary of the Treasury shall from time to time direct, to the payment of the public debt.

Let Dx denote the amount of the public debt outstanding at any time (x); and let Fx denote the aggregate amount of the sinking fund as accumulated to that time:

Then will

$$Dx + Fx = \text{a constant quantity,}$$

and therefore

$\Delta Fx = -\Delta Dx = 0$; the symbol Δ denoting annual increment.

But $\quad \Delta Fx = F(x+1) - Fx = .01Dx + .05Fx$

Therefore,

$$\Delta F(x+1) - \Delta Fx = .01\Delta Dx + .05\Delta Fx$$
$$= -.01\Delta Fx + .05\Delta Fx$$
$$= .04\Delta Fx$$
$$\text{and } \Delta F(x+1) = \Delta Fx\ (1.04)$$

It follows that

$$\Delta F(x+n) = \Delta Fx\ (1.04)^n$$

and

$$F(x+n) - Fx = \Delta Fx\left(1 + 1.04 + \overline{1.04}^2 + \ldots . \overline{1.04}^{n-1}\right)$$
$$= \Delta Fx \left(\frac{\overline{1.04}^n - 1}{.04}\right)$$

$$\text{Therefore } \overline{1.04}^n = \frac{.04\,(F(x+n) - Fx) + \Delta Fx}{\Delta Fx}$$

To illustrate by a practical example:

Required the period in years necessary to cancel in accordance with the provisions of law, \$750,000,000 of the public debt; the amount of debt outstanding, (less cash in Treasury) on the 1st of January, 1880, being \$2,011,798,504.87, and the amount of the sinking fund accumulated up to that date \$472,243,622; assuming the future rate of interest which may be realized on investments to be *five* (5) per cent. per annum.

$$Dx = 2{,}011{,}798{,}504.87$$
$$Fx = 472{,}243{,}622.—$$
$$\text{and } \Delta Fx = .01\ Dx + .05\ Fx$$
$$= 20{,}117{,}985$$
$$+\ 23{,}612{,}182$$
$$= 43{,}730{,}067$$
$$\text{and } F(x+n) - Fx = 750{,}000{,}000$$

Therefore $\overline{1.04}^n$ which equals

$$\frac{.04\,(F(x+n) - Fx) + \Delta Fx}{\Delta Fx} = \frac{73{,}730{,}067}{43{,}730{,}067}$$

$$\text{Therefore } n = \frac{.226864}{.0170333} = 13.2993 \text{ years.}$$

If investments and re-investments are to be improved *semi-annually* (instead of annually) at the rate of 5 per cent per annum, the formula would become

$$\overline{1.02}^{2n} = \frac{73,730,067}{43,730,067}$$

and therefore

$$n = \frac{.226864}{.017200} = 13.190 \text{ years},$$

the period required to cancel $750,000,000 of the public debt.

The next communication was by Mr. T. N. GILL on

SOME REMARKABLE INSTANCES OF INGESTION AMONG FISHES.

[Abstract.]

Apocryphal as it may appear, there are some fishes which are capable of ingesting and stowing away entire, others several times larger than themselves. This extraordinary feat is rendered possible in the first place by the great size of the mouth which is cleft far backwards, and in the next by the excessive distensibility of the stomach and abdominal integuments. The captor seizes the larger fish by the tail and climbs over it as it were by alternate movements of the upper and lower jaw, until finally the entire animal is stored in the stomach. Meanwhile, the stomach and of course the adjoining soft parts become more and more distended, and hang down like an enormous sack. The most remarkable examples of such capacity have been found in the *Chiasmodon niger*, a species related to the cod family, but several others, especially representatives of the Lophiid and Ceratiid families, are likewise prone to attack others larger than themselves.

179TH MEETING. APRIL 10, 1880.

The President in the Chair.

Fifty-two members present.

The first communication was by Mr. WILLIAM HARKNESS

ON THE SOLAR CORONA.

[Abstract.]

Mr. HARKNESS' remarks were based chiefly upon his own obser-

vations of the total eclipse of 1878, which were made at Creston, Wyoming.

One special object on that occasion was to search for lines in the ultra-violet part of the spectrum. The eye-piece of the spectroscope-telescope was removed and a fluorescent eye-piece substituted. The spectrum used was that of the second order, and upon examining the ultra-violet region prior to totality, it was found remarkably distinct. During totality, however, neither bright lines nor continuous spectrum were seen. It may be that the corona has no ultraviolet spectrum, but these observations are not conclusive on that point. It may be so faint as to have been invisible because of the great waste of light by the use of a diffraction grating. Probably it would have been better to have used the first order spectrum instead of the second, but the knowledge we now possess points to the conclusion that a crown glass prism would be superior to a diffraction grating.

Prof. HARKNESS also described in detail the operations of photographing the corona during totality. A point which it was desirable to investigate with care was the determination of coefficients to be used in formulæ expressing the time of exposure of a photographic dry-plate under such circumstances. If we denote the focal distance of the camera objective by F; its working aperture by d; the exposure coefficient by C; and the length of the exposure in seconds by t, then

$$t = C\left(\frac{F}{d}\right)^2$$

In the present instance the negative which had an exposure coefficient of 1.505 seems to exhibit nearly or quite all of the corona which is visible to the naked eye, excepting only the streamers, yet as the whole series of negatives taken on this occasion, shows that every increase of exposure produced a corresponding increase in the size of the image of the corona, there is no certainty that the final limit was attained. It seems desirable during the next eclipse to push the coefficient to a value of 5, and greater if possible. This can only be done with the most rapid portrait lenses, and Prof. HARKNESS believed that the objectives employed should have a ratio of aperture to focal distance of not less than one-fourth to one-half. But in passing outward from the moon's limit, the light of the corona diminishes very rapidly, and hence to depict all its

parts, a series of negatives is absolutely necessary, commencing with very short exposures, and ending with very long ones.

The process of obtaining drawings of the negative was as follows: First, an enlarged positive was made. Upon this is laid a transparent gelatine film, and the whole is placed in a strong light. The details of the image were scratched upon the gelatine film with a needle-point. The film being removed lead pencil dust is rubbed into the engraving, and an impression made upon paper. This picture is a reverse of the proper outlines, but a second gelatine tracing restores everything to the normal order.

The measurement of the brightness of the corona is surrounded with difficulties, drawings from photographs are untrustworthy. Comparison of original negatives would be better, but this is liable to much uncertainty. If, however, we have for any eclipse a series of photographs, and also a determination of the total light by means of a Bunsen photometer, data will have been provided for an approximate comparison with future eclipses. An application of this process to the present eclipse showed that the intensity of the coronal light varied inversely with the square of the distance from the sun's limb. Prof. Harkness' conclusions respecting the total light of the corona of July 29th, 1878, are as follows:

1. The total light of the corona was 0.072 of that of a standard candle at one foot distance; or 3.8 times that of the full moon; or 0.0000069 times that of the sun.

2. The photographs show that the coronal light varied inversely as the square of the distance from the sun's limb.

3. The brightness of every part of the corona is given quite approximately in terms of the brightness of the full moon by the expression

$$B = \frac{0.15}{h^2} (23' + 100' \cos \theta)$$

in which h = distance from the sun's limb in minutes of arc and θ = the latitude of a point measured from the sun's equator. For very small values of h this formula fails. Probably the brightest part of the corona was about 15 times brighter than the surface of the full moon or 37000 times fainter than the surface of the sun.

4. The corona of December 22d, 1870, seems to have been 7½ times brighter than that of July 29th, 1878.

In deducing these results visual and photographic data have been intermingled as if they were homogeneous. Such a procedure is certainly open to objection, but it is not likely that it introduced any error grave enough to impair the value of the results as first approximations.

The next communication was by the President of the Society Mr. Simon Newcomb on

THE PRINCIPLES OF TAXATION.

This paper was reserved by the author. After remarks upon this communication by Mr. Edward Atkinson, of Massachusetts, and Mr. E. B. Elliott the Society adjourned.

180th Meeting. April 24th, 1880.

The President in the chair.

Forty-three members present.

The order of exercises for the evening consisted in the following communications: (1) by Mr. A. A. Michelson, U. S. N., on the modifications to which light is subject in passing through a very narrow slit; (2) by Mr. Edgar Frisby remarks on the late solar eclipse observed in California; (3) by Mr. H. M. Paul on earth tremors, as shown by astronomical observation; and (4) by Mr. E. S. Holden remarks on Gould's Uranometria Argentina.

Mr. Michelson's communication on

THE MODIFICATIONS SUFFERED BY LIGHT IN PASSING THROUGH A VERY NARROW SLIT,

was substantially as follows:

When the sun is viewed through a narrow slit, the diffraction fringes are observed. On making the slit narrower, the following phenomena are presented. When the width of the slit is $.01^{mm}$ the light acquires a faint-bluish tint, and on viewing it with a Nicol's prism, traces of polarization are evident. When the light is faintest, the bluish tint is more marked. When the width of the slit is $.005^{mm}$ the blue tint is more marked and the polarization is quite distinct. When the light is very faint, the tint is deep blue. When the width is $.001^{mm}$ the tint changes to violet and the polarization is complete. As the light grows fainter, the violet tint

becomes more marked. If the slit and the Nicol be interchanged, the same results follow in the same order. (*See page* 148.)

The experiment may be varied in a striking manner by substituting a double-image prism for the Nicol, when the two images may be compared side by side. The experiments are somewhat trying on account of the faintness of the light. The principal conditions to be fulfilled are as follows: (1.) The sun is to be viewed through the slit, the latter being placed as near as possible to the eye. (2.) The double-image prism should be used in order to compare the two images side by side. (3.) The slit should be from .01^{mm} to .001^{mm} in width. (4.) The slit should be as nearly perfect as possible.

These experiments seem to show, 1st, that light which passes through a very narrow slit is polarized in a plane at right angles to the slit; 2d, that the shorter waves pass through more readily than the longer.

On this communication Mr. NEWCOMB remarked that there was a temptation here to inquire whether the phenomena recited might not have a bearing upon the unsolved problem of the amplitudes of luminous vibrations. Mr. W. B. TAYLOR entertained the view that these amplitudes were so small that even the narrow slit used by Mr. Michelson would bear too large a ratio to them to exercise any appreciable affect. No explanation of the phenomena was suggested.

Mr. H. M. PAUL then remarked on

EARTH TREMORS AS SHOWN BY ASTRONOMICAL OBSERVATIONS.

[Abstract.]

The observations referred to were made with a view of determining to how great an extent certain localities in the vicinity of Washington city were liable to tremors during the passage of railway trains, and they were undertaken in connection with the question of the selection of a new site for the Naval Observatory. The mode of inquiry was by observing with a 3¾ inch telescope the effect of such tremors upon a basin of mercury in a manner similar to observations for determining the meridian alignment of transit telescopes. In these cases the pole star was used. The magnifying power was about 185. The mercury well was 14 inches by 10 inches, and the mercury amalgamated with tin, to within about two-

thirds of saturation. Although no wind was perceptible, the naked surface of the well was so unsteady as to give no definition, and it was necessary to cover it with plate glass. Even then a very large number of images were produced having a checker-board arrangement. By careful manipulation these images were at length reduced to rows of points.

Observations were made at the times of passage of trains. At a distance of 0.29 of a mile, a fast express train produced an appearance of boiling, the bright points being converted into a patch of nebulous light. A slow train at the same distance produced only a radiation from the bright points. At greater distances, phenomena of somewhat different character were observed. It appeared, however, that the effects of tremor were not proportional to the distance, which must be attributed to inequalities in different portions of the earth in their power of transmitting vibrations. At 0.84 of a mile there was only a slight boiling from the passage of a fast train, and a slight radiation of the points. At 0.94 of a mile, only radiation was observed. At 0.81 of a mile there was less effect from the fast train than from the slow train at any of the other localities.

The next communication was by Mr. Edgar Frisby.

REMARKS ON THE TOTAL SOLAR ECLIPSE OF JANUARY 11TH, 1880.

[Abstract.]

Perhaps the phenomenon of the total eclipse of the 11th of January, was more interesting on account of its comparatively negative character than anything else; the central line passed through the most rugged part of the coast of California: about 150 miles south of San Francisco, it was observed from the top of the Santa Lucia mountain, a peak about 6,000 feet above the level of the Pacific ocean, it was of very short duration, only about 30 seconds; one fact all the observers appeared to notice, that was, as the moon's limb advanced over the sun, it was a little darker than the surrounding sky near the sun. During totality it was hardly as dark as I expected, we could barely discern Jupiter and Mars, and no fixed stars were visible, a bright concentric corona was seen round the sun hardly one-third part of the sun's diameter in breadth and nearly uniform, but no outer corona and streamers such as are usually described were visible, probably owing to the short time of duration of the total phase.

The last communication was by Mr. E. S. HOLDEN, consisting of a review of Prof. B. A. Gould's Uranometria Argentina. This was the substance of a paper published by Mr. Holden in the International Review for April, 1880.

At the conclusion of Mr. HOLDEN'S remarks, the Society adjourned.

181ST MEETING. MAY 8TH, 1880.

Vice President J. E. HILGARD in the chair.

Thirty-four members present.

The presiding officer stated to the Society that it was his sad duty to announce the death of an honored and beloved fellow-member Mr. JONATHAN HOMER LANE, who died in this city, May 3d, 1880. Having been intimately associated with him for the last ten years, and having an acquaintance with the facts of his career for the last thirty years, Mr. Hilgard felt it incumbent upon himself to take the initiative in commemorating his character before the Society. Mr. Lane was not personally known to many persons except his intimate friends. He was very diffident in manner and possessed little faculty of speech, though when he spoke, he spoke with a rare logic which carried conviction. He was born over sixty years ago in the northern part of New Hampshire. His father was a farmer, who, though he possessed little wealth, was yet able to give his sons such education as the neighborhood could afford. Our lamented associate, through his rare natural gifts, was able to prepare himself for Yale College, where he graduated with honor. He obtained the means of subsistence by teaching. While in college he was noted for the same clear-minded and logical qualities which distinguished him in after life, and Mr. Hilgard, had heard his fellow-students testify to the obligations they were under for the assistance which he had rendered them in their studies, especially in those involving mathematics and natural science. On leaving college he became attached to the Coast Survey, where he rendered valuable service, and soon made his mark by displaying originality and discriminating judgment.

When the enlargement of the scope and working force of the Patent Office took place in the year 1846, he was, through the recommendation of Prof. Joseph Henry, appointed to a position,

and was honorably associated with what may be called the second stage of that important bureau—a stage in which many of the most important inventions of the century were patented and developed. He remained in that position for about ten years, and having accumulated some means he was then induced to withdraw, and act as expert counsellor in patent cases, while devoting much of his time and means to researches in physical science. He felt called upon, however, to devote a part of his means to the assistance of two sisters and a brother. He was exceedingly generous in his disposition, and in order that he might be able to render this assistance he refused to marry; but his generosity was at the cost of what he valued most, the means and facilities of research. During the period of his investigations he enjoyed in a high degree the confidence of Prof. Henry, who gave him warm encouragement. His experiments were attempts to determine the absolute zero of temperature by the expansion of gases; but he never published his results though the experiments were based upon correct principles, and must be considered as in the main successful. His failure to publish them is attributable to a desire to attain further refinements and closer approximations, and also, in a great measure, to the exceedingly exacting standard which he always set up as the criterion of scientific logic.

A few years ago, when Congress authorized the construction of metric standards, and when the death of Mr. Saxton left a vacancy, Mr. Hilgard was so fortunate as to secure the services of Mr. Lane, and retained them until the latter's death. The report to which Mr. Lane contributed is not yet printed, but happily it is completed, and will soon be published.

The quality of Mr. Lane's mind was truly remarkable. It was chiefly characterized by an extraordinary precision of thought and of logic; but it was coupled with a great want of fluency of speech. It was in his writings that this clearness of mind became manifest;—so lucid was it, and so fully were those qualities of soundness and precision appreciated, that his co-laborers never thought a difficult induction safe until it had passed through the alembic of Mr. Lane's criticism. By others he was no doubt equally appreciated.

In conclusion, Mr. Hilgard offered the following resolution, which was unanimously adopted:

Resolved, That the Philosophical Society of Washington place

on their record an expression of the high esteem in which they have long held their late fellow-member, Jonathan Homer Lane, for his valuable services to science, as well as for his high intellectual and moral character.

Mr. William B. Taylor said that though he was unprepared to pay a just tribute to the memory of our departed associate, he was impelled to unite with Mr. Hilgard in testifying to his many noble qualities both of mind and character. From long acquaintance with him and his work, he could fully endorse all that the last speaker had said. He had rarely enjoyed the privilege of meeting with a mind so clear, transparent, and logical, or one capable of such a full apprehension of the principles of science, and such a keen perception of their consequences. The most striking quality was the extreme precision of his thoughts, and it had seemed to him that this very quality may have in a great measure, led indirectly to the hesitation of speech which characterised Mr. Lane's address. Words to him were seldom the exact exponents of thought, and his care in the selection of language which might express with precision the tenor of his thoughts, no doubt led to his hesitation; but whenever difficult and doubtful questions arose, the clearness of his insight and the soundness of his views were such as belong to few minds. Not only was he a very fine mathematician, but he possessed in a high degree the much rarer qualifications essential to the mathematical physicist.

Mr. Cleveland Abbe remarked upon the communication of Mr. Albert Michelson, made at the previous meeting, as follows:

He believed that an explanation of the phenomena could be given, and if it be correct, it opens a field of observation as important as the experiments of Crookes upon the behavior of gases under an approximate vacuum. The narrowness of the slit indicates that we have indeed some novel conditions of the air to deal with. In the present instance, light is not passing through an ordinary gas, but through the layer of gas, which according to optical theories, is under a state of stress because it is contiguous to a solid surface; in short, that layer that produces ordinary polarization by reflection. This layer is always present, adjacent to the jaws of the slit, and by narrowing the slit to the extreme limits used by Mr. Michelson, we exclude the great mass of gas in its ordinary state, and have to do only with the polarizing

layers. By introducing various gases and liquids between the jaws of the slit, and by utilizing the influence of electrical or magnetic stress, or by other devices, the true explanation may be possibly ascertained.

Mr. WILLIAM B. TAYLOR further observed that the experiments of Mr. Michelson were certainly very original and beautiful, and the problem does not seem to be soluble by any known data. Mr. Abbe's explanation seemed to be quite plausible. Between the jaws of a slit only .001^{mm} in diameter, the interval is probably near the limits of mean molecular excursions, and the condition of air or any gas under those circumstances would be a novel one to contemplate. The exclusion of some waves of light observed by Mr. Michelson might be attributable to the disturbing behavior of molecules so situated and restricted in their motions.

Mr. J. W. OSBORNE stated that when this paper was presented to the National Academy of Sciences, Mr. Michelson had alluded to an explanation of similar purport to that of Mr. Abbe, but had rejected it as being untenable.

The Chair announced to the Society the election to membership of Dr. JEROME H. KIDDER, U. S. N., and FERDINAND AUGUSTUS HASSLER.

The first communication for the evening was from Mr. G. K. GILBERT on

THE DRAINAGE SYSTEM OF THE BLACK HILLS OF DAKOTA.

[Abstract.]

Mr. GILBERT stated that it had never been his privilege to see the Black Hills; it had, however, been his duty to prepare for publication and edit the work of the lamented young geologist, the late Henry Newton, which had been left by him in the form of copious notes and unrevised manuscript. This work is an account of the geology of the Black Hills derived from Mr. Newton's studies while examining that district under the direction of the Interior Department.

The Black Hills form an isolated district of mountain and plateau rising out of the great plains of Dakota, and are separated from other mountain ranges by a wide interval, the nearest being the Big Horn mountains to the westward. They are thus well

suited for a monograph, and thus it was that Newton discussed them.

The general form of the group was exhibited by an illustration conveying the appearance they might present to the eye of an observer elevated many miles above the extreme southern end. The sedimentary rocks from the Potsdam sandstone up to the summit of the Cretaceous, are flexed upwards towards the hills from all directions. Upon the western side, the Carboniferous strata after being flexed upward, flex back to approximate horizontality upon the summit of the uplift, while later beds are cut off and expose in succession their upturned edges upon the flanks of the hill region. The eastern part of the uplift is denuded of even its palæozoic beds, and exposes a mass of contorted Archæan schists.

The drainage of the entire area is collected into two streams, the Belle Fourche and the North Fork of the Cheyenne. These streams cross the south and north extensions respectively of the Black Hills uplift, cutting profound valleys in so doing. The tributaries of these streams all head around a central axis in the western or plateau part of the region, and flow thence in all directions, gradually gathering into the two forks. It now becomes of interest to inquire how far the positions and courses of these streams have been dependent upon the deformations which the strata have undergone, and how far upon antecedent causes. With this view, streams may be considered as belonging to two classes—1st. Consequent streams, which result from the formation of the slopes down which they run, and have had their courses determined by such slopes. 2d. Inconsequent streams, or those which run independently of existing slopes or dips, and which could not have been influenced as to their positions in any very important manner by such slopes. Again, these inconsequent streams may be subdivided into two groups. 1st. Those which had their courses marked out prior to the formation of the structural deformations through which they run, regardless of the dip or slope of the strata. Such streams may be called antecedent drainage, having been laid out before the formation of the uplifts, and having successfully maintained their prior right of way by cutting down their beds as fast as the obstacles were elevated athwart their courses. 2d. Inconsequent streams may have been laid out upon the surface of strata which overlie inconformably much more ancient beds. The upper strata being in time denuded, the streams which at first were consequent, cut

down into these older beds to which their relation becomes an inconsequent one. These streams may be called superimposed drainage.

The creeks that drain the Black Hills constitute a consequent drainage system of rare perfection. Rising near the center of the uplift, they follow the direction of the dip on all sides, and hold independent courses until they have passed outside the area of disturbance. The symmetry of their arrangement indicates that the formation of the arch began under water, so that its original surface was not furrowed by a pre-existent drainage, but presented to meteoric waters, when it was finally laid bare, an even curvature the slope of which everywhere represented the dip. The creeks came into existence when the summit of the arch appeared above the water, and its axial line became at the same time a watershed. As its slopes gradually emerged the creeks extended their lower courses, but their upper courses remained the same, and so did the watershed. With the progressive degradation of the arch, the creeks have descended vertically from their first position to their present without essential modification of their horizontal relations, and we have no reason to doubt that the watershed of to-day is directly beneath the position of the original watershed.

The watershed therefore marks the place of the original axis of deformation, and as it does not correspond to the present axis of deformation but lies fifteen miles further west, it is concluded that the growth of the arch was not uniform, but was at first more rapid on one side and afterward on the other.

The discovery of this irregularity of movement does not make the Black Hills displacement an exception, but rather helps to ally it to the other displacements of the west, the history of which, as far as known, is never simple.

The rivers which embrace the hills and receive the waters of the creeks are of more recent date, and are superimposed on the uplift. After the arch was formed, and to some extent degraded by erosion, a lake came into existence, which surrounded its base, and remained long enough to partly bury its flanks with lacustrine sediments, the sands and clays of the White River Miocene. When the lake finally disappeared a new drainage system was created on its bed, and to this system belong the branches of the Cheyenne. Subsequent degradation has carried away the lake beds from the immediate vicinity of the Black Hills, but the rivers inaugurated

on them have persisted. They were consequent to the lake beds, but they have cut their way down into the upbent rocks, and to their structure they are inconsequent by superposition.

At the conclusion of Mr. Gilbert's paper the Society adjourned.

182D MEETING. MAY 22D, 1880.

The President in the Chair.

Thirty-two members present.

The minutes of the last meeting were read and adopted.

A paper was then read before the Society by the Rev. J. OWEN DORSEY

ON THE GENTILE SYSTEM OF THE OMAHAS.

The Omahas belong to the Dakotan family. That family is divided into six or seven groups, which are as follows: I. The Dakota, comprising the tribes of Dakotas and Assiniboins. II. The Dhegiha, including the Ponkas, Omahas, Quapaws, Osages, and Kansas. III. The Winnebago group, embracing the Winnebagoes, Iowas, Missouris and Otoes. IV. The Mandan. V. The Hidatsa and Crows. VI. The Tutelo, now in Canada; and to these some add a seventh group, the Katâba of South Carolina.

The Omahas, to the consideration of whom this essay is limited form a tribe of the Dhegiha group. This group consists of Upper Dhegiha or Omahas and Ponkas; and Lower Dhegiha or Quapaws, Osages and Kansas.

Dhegiha means "belonging to the people of this land." It answers to the Otoe, ‘Ciwere, and to the Iowa, ‘Ce‘kiwere.

If an Omaha or Ponka be challenged in the dark when on his own land, he will reply "I am a Dhegiha." A Kansas, on his own land will say, "I am he who is a Dhegiha." But when away from home, even when on the land of a tribe of the same group, the man must give the *tribal* name, saying "I am an Omaha," "I am a Ponka," or "I am he who is a Kansas."

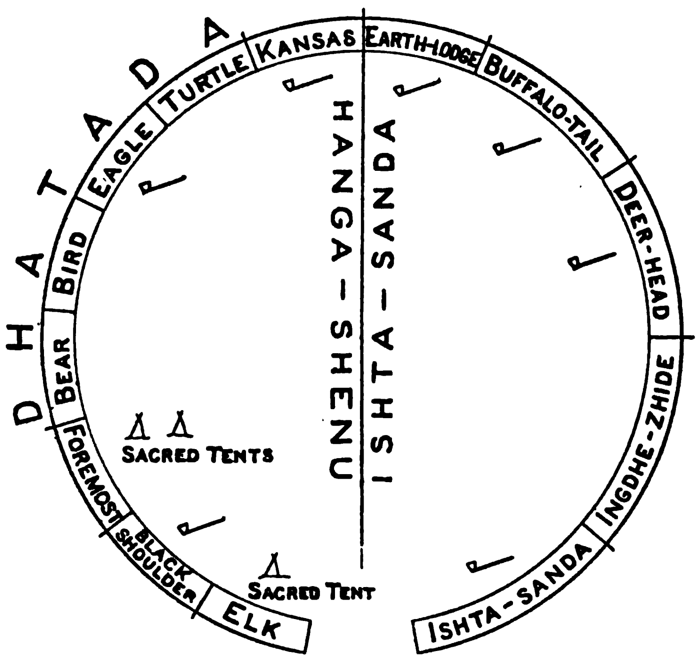
Omaha, or rather U-ma^{n}'-ha^{n}, means "up-stream people." They say that in former days their ancestors were on the Ohio river. When they reached the mouth, they crossed the Mississippi. There the people separated. Some went down the river, and became

Ugakhpa, Quapaws, or Down-stream people; while the rest travelled up the Mississippi, and were called U-manhan, or "they who went against the wind or current."

While on the hunt in the olden time, and whenever their camping place was small, the Ponkas encamped in three concentric circles; and the Omahas, who were a smaller tribe, camped in two such circles.

Hence, the Dakotas called the Ponkas "The Three Nations," and the Omahas "The Two Nations." But when they could find a large camping ground, each tribe had but one circle, as they have at present.

Diagram No. I.—THE OMAHA TRIBAL CIRCLE.



Note.—In this diagram the gentes that keep the sacred tents, and those that hold the seven sacred pipes, are designated by the proper signs.

The Omaha circle is divided into two parts. The five Hangashenu gentes camp in the first, and in the second are the five Ishtasanda gentes. The Hangashenu claim to be the superior people.

Their gentes are Elk, Black-shoulder (a buffalo gens), Foremost (a buffalo gens), Dhatada (Thunder people?) and Kansas.

The Ishtasanda gentes are—Earth-lodge makers (or Wolf people), Buffalo-tail, Deer-head (or Deer people), Red dung (a buffalo gens), and Ishtasanda (a Reptile and Thunder people).

Orgin of Gentes.—The Black-shoulder people say that their ancestors were buffaloes, who lived in the water. When they came out of the stream, they made the water muddy, hence the first-born son is called "Makes the water muddy." Having reached the land, they snuffed at the four winds, and prayed to them. The north and west winds were good, but the south and east were bad.

The ancestors of the Foremost gens were buffalo, and lived under the water. They moved about with their heads bowed down and their eyes closed. By and by they opened their eyes in the water, hence their first name "Opening the eyes in the water," which is also the birth name of the first-born son. Emerging from the water, they lifted their heads and saw the blue sky for the first time, so they took the name "Clear sky makers."

The Ingdhe-zhide or "Red dung" gens has been so called since the visit of the seven old men with the sacred pipes. When they reached the camp of this gens, they found the erect body of a buffalo, partly buried in the ground, visible from the flanks up. The animal had been flayed, and the skin was made into a tent. The body was bloody and frightful to behold, so the old men passed by without giving them a pipe.

The only thing pointing to the mythological origin of the Elk gens is the sacred bag of that people. This bag is the skin of a prairie-wolf, and contains a clam or oyster shell, the bladder of a male elk filled with killickinnick, some tobacco leaves, a pipe, a cedar stick, and a piece of the sinew of a male elk.

Rights and duties of each gens.—(*a.*) Seven have the sacred pipes: Black-shoulder, Dhatada, Kansas, Wolf or Earth-lodge, Buffalo-tail, Deer-head, and Ishtasanda.

Elk keeps the war tent, and leads in the worship of the thunder-god.

Foremost lights the sacred pipes for the chiefs, and keeps the two sacred tents. He regulates the buffalo hunt.

(*b.*) Law of membership. A child belongs to the gens of the father; hence, half-castes have no status in the gens.

(*c.*) Law of marriage. A person must marry outside of the gens, and under certain restrictions which cannot be given in this paper.

(*d.*) Law of things prohibited to be touched or eaten. In some cases this governs the whole gens, in others, each sub-gens has its especial taboo. Thus, the Elk people cannot touch or eat any part of the male elk or deer.

(*e.*) Religious ceremonies peculiar to each gens or sub-gens—Among these are the naming of an infant, and the worship of the thunder-god.

In the Deer-head gens, the first ceremony is conducted as follows: All the members of the gens assemble on the fifth day after the birth of the child. Those men belonging to the sub-gens of the infant cannot eat any thing cooked for the feast, but the men of the other sub-gentes are at liberty to partake of the food. The infant is placed within the circle of the gens, and the privileged decoration is made on its face.

Taking some red-clay paint, two parallel lines are drawn across the forehead, two down each cheek, one across the face over the mouth, and one under the mouth; then, with three fingers of the right hand, red spots are made down the back of the child, at short intervals, in imitation of the fawn. The child's breech-cloth is so marked. On its arms and chest are rubbed stripes as long as the hand. All of the Deer-head people in attendance, even the servants, decorate themselves, rubbing the rest of the red paint on the palms of their hands, they pass their hands backward over their hair; and they finally make red spots on the chest about the size of the palms of their hands.

The members of the Pipe sub-gens, and those persons in the other sub-gentes who are related to the infant's father through the calumet dance, are the only ones who are allowed to use the privileged decoration, and to wear fine feathers in their hair. If the child belongs to the Pipe or Eagle sub-gens, charcoal, blue-clay and the skin of a wild-cat are placed beside him, as the articles not to be touched by him in after life. Then they say to him "this you must not touch; this too you must not touch, and this you must not touch." The blue clay symbolizes the blue sky.

[Now according to the Iowa myth, the Eagle people came down from the sky.]

Worship of the Thunder-God.—When the first thunder is heard in the spring of the year, Elk invites Bear to a feast. On his arrival Bear opens the sacred prairie-wolf bag, takes out the pipe and the elk bladder tobacco-pouch, which Elk dare not touch. Bear then takes some tobacco or killickinnick from the pouch, and fills the pipe. The lighted pipe is held up towards the sky, and the Thunder-god is thus addressed:

"Well! venerable man! by your striking you are frightening us, your grand-sons who are here. Depart on high."

Then all present smoke the pipe, and join in the feast. It is alleged that at the conclusion of the feast the rain always ceases, and the Bear people return to their homes.

(*f.*) Style of wearing the hair. The boys of each gens are obliged to wear their hair in a prescribed style. The Buffalo people have four long locks, two on each side, in imitation of the buffalo.

The Bird people have four locks, representing the head, tail, and wings of a bird.

The Turtle people have six locks, in imitation of the head, tail, and legs of a turtle.

(*g.*) There are five classes of relationship, as follows: 1. In the gens; 2. By consanguinity (including the members of other gentes); 3. By marriage; 4. By the taboo of the gens or sub-gens; and 5. By the calumet dance.

(*h.*) Each gens has a list from which a father is expected to select the names for his children.

These are called nikie or sacred names, and generally refer to some act of the mythical ancestor, or to some part of his body.

There are, in addition to these, seven names sacred above all others, as referring to the mythical origin of the gens, and which used to be conferred upon the sons born into each household; and there were as many similar birth-names for the daughters.

These birth-names should not be confounded with the household names of the children.

Peculiar customs of the gens.—I give two examples. The Kan-

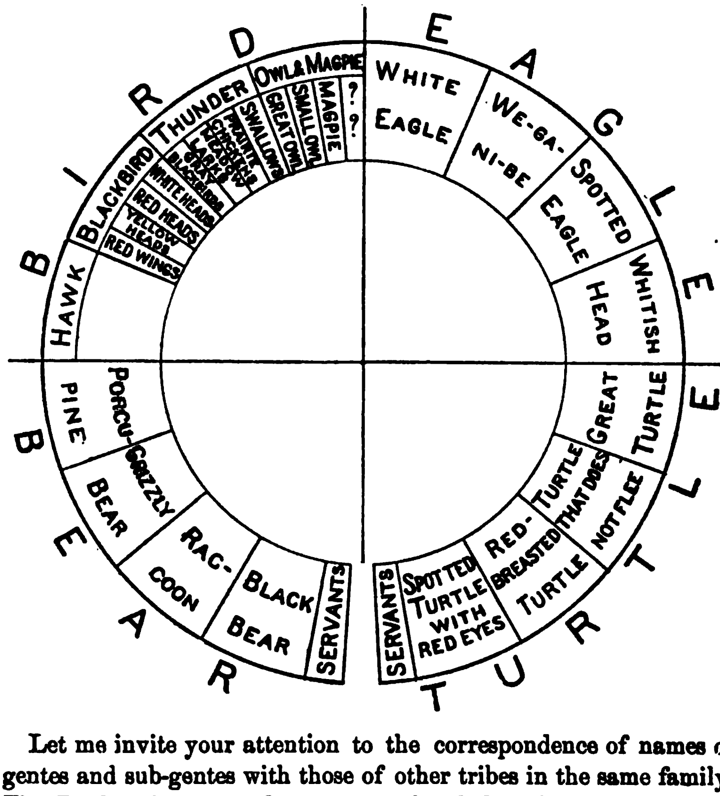
sas Wind people flap their blankets to and fro, in order to raise a wind that will drive away the mosquitoes.

When the corn-fields of the Ishtasanda gens are troubled with a species of worm, the men proceed as follows:

They heat some grains of corn, and then pound up with this some of the worms.

They make a soup of the mixture, and when that is eaten, they imagine that they will have no more trouble.

Diagram No. II.—THE DHATADA GENTILE CIRCLE.



Let me invite your attention to the correspondence of names of gentes and sub-gentes with those of other tribes in the same family. The Ponkas have an Osage gens; the Oglala Dakotas have an Osage section; among the Omahas is a Kansas gens; some of the Kansas form the Ponka gens; and I have been told that some of the Wisconsin Winnebagoes call themselves the Omahas.

When a gens assembles in its own circle, the servants are stationed on each side of the entrance, and the rest of the circle is divided between the sub-gentes.

Sub-gentes.—Each gens is composed of four sub-gentes, some of which in turn are divided into four sections; and these sections, in some cases, are divided into sub-sections.

The Elk sub-gentes are: 1. Elk; 2. Keepers of the war tent; 3. Thunder; and 4. One whose name is unknown to me. The Black-shoulder sub-gentes are: 1. Keepers of the Pipe (Eagle people,) or they who eat no red corn; 2. They who touch no charcoal; 3. They who eat no buffalo tongues, and touch not a buffalo head; and 4. The Criers or Heralds.

The Foremost sub-gentes are: 1. They who eat not the sacred buffalo meat; 2. Real Foremost, they who eat no buffalo tongues; 3. Servants of the Sacred Tents; and 4. They who touch not "Blue skins."

The Dhatada sub-gentes are: 1. Bear; 2. Bird; 3. Eagle; 4. Turtle.

Two of the Kansas sub-gentes are: Keepers of the Pipe, and the Wind people.

The Earth-lodge sub-gentes are: 1. Prairie-wolf and Wolf; 2. Keepers of the sacred stone; 3. Keepers of the pipe; and 4. They who touch not the white swan, (this last is not a taboo.)

Two of the Buffalo-tail sub-gentes are: 1. Keepers of the Pipe, or they who touch not the lowest rib of a buffalo; 2. Keepers of the sweet medicine, or they who touch not a buffalo calf.

The Deer-head sub-gentes are: 1. Keepers of the Pipe (Eagle and Thunder people), or they who touch not charcoal, blue clay, and the skin of a wild-cat; 2. They who touch not charcoal, and the fat of a deer, and who can not wear deer-skin moccasins; 3. Deer; and 4. Thunder people.

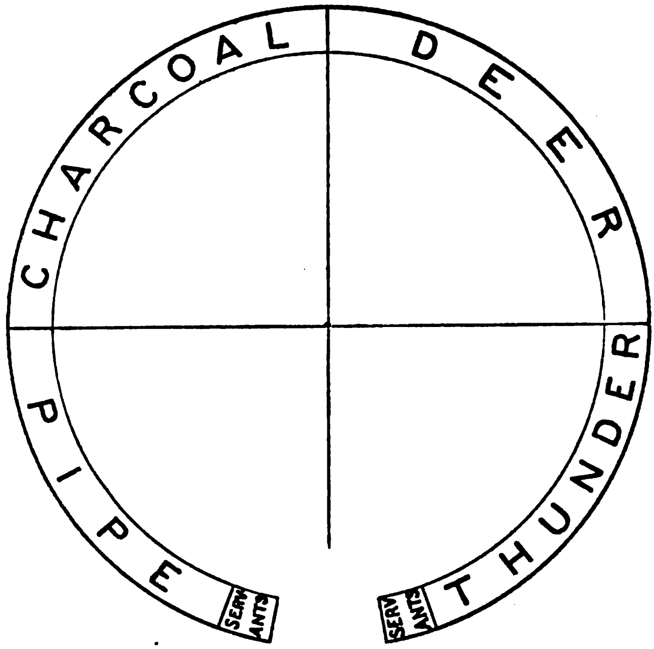
The names of the four Red-dung sub-gentes are unknown to me, though I have obtained the names of their chiefs.

The Ishtasanda sub-gentes are: 1. Keepers of the claws of the wild-cat (Thunder people); 2. Real Ishtasanda or Reptile people; 3. Keepers of the clam shell or Thunder people; 4. Keepers of the sacred pipe.

Origin of the sub-gentes.—The Eagle people of the Dhatada

gens are known as "They who touch not the buffalo head or skull." This name originated when the seven old men carried the sacred pipes around the tribal circle.

Diagram No. III.—The Deer-Head Gentile Circle.



The Eagle people found that they were slighted, and started away in anger, determined to abandon the tribe. But the old men pursued them, and handed them a bladder filled with tobacco, and also a buffalo skull, saying, "Keep this skull as a sacred thing." Hence the name of the sub-gens, and also "Dried or withered Eagle," meaning "Dried buffalo skull," the birth-name of the first-born son.

Rights and duties of sub-gentes.—The second Elk sub-gens keeps the war-tent and sacred bag, and leads in the worship of the Thunder-God. The members of the first sub-gens of the Hanga, or Foremost, keep the Sacred Bark lodge, the most sacred of the three, which contains the sacred pole of the Ponkas and Omahas. While they cannot eat the sacred buffalo meat, they can eat the

tongues. The members of the Real Hanga sub-gens keep the sacred lodge made of the skin of a white buffalo. This lodge regulates the buffalo hunt. These people can eat the sacred buffalo meat, forbidden to the first sub-gens; but they are not allowed to eat the tongues.

The members of the Bear sub-gens assist the Elk people in the worship of the Thunder-God.

The Raccoon section of this sub-gens is called Khu'ka, because its members are the *singers* for those who dance.

The Eagle people keep the sacred buffalo skull, and fill the seven sacred pipes for the chiefs.

The Pipe sub-gens of the Black-shoulder gens, being Eagle people, are entitled to use Eagle birth-names, as well as the Buffalo birth-names of the gens. So the Pipe sub-gens of the Deer-head people have Eagle birth-names, as well as the Deer birth-names of the gens

The Bird people have a curious custom. When the birds eat the corn before harvest, the men of this sub-gens, go into the field and roast several ears of corn, and chew the grains, which they spit out all around them. They imagine that such a procedure deters the birds from making further inroads upon the crop.

When there is a fog, the Turtle people draw the figure of a turtle on the ground. They place at its head, tail, and at each of its feet, pieces of tobacco, which they cover with small pieces of a red breech-cloth. This, they say, causes the fog to disappear very quickly.

The following are sections and sub-sections of sub-gentes.

Those of the Bear are: 1. Black bear; 2. Raccoon or the singers; 3. Grizzly bear; and 4. Porcupine.

The Bird sub-gens is divided into, 1. Hawk; 2. Blackbird; 3. Thunder; 4. Owl and Magpie.

The Blackbird sub-sections are: 1. Red-wings; 2. Yellow-heads; 3. Red-heads; and 4. White-heads.

The Thunder sub-sections are: 1. Gray blackbird; 2. Meadow-lark; 3. Prairie-chicken; and 4. Swallow.

The Owl sub-sections are; 1. Great Owl; 2. Small Owl; 3. Magpie; and one whose name has not been given me.

It has been suggested that the ten secondary divisions of the Omahas are *tribes*, and that what I have termed *sub-gentes* are the *real gentes*. But this is inconsistent with the Indian terminology.

The "camp-fire," whether among the Dakotas, Winnebagoes, or Omahas, is the "gens." And among the Omahas this "camp-fire" has another name, Uba'na^n, meaning "A clump of shoots from a common stock or root;" and a sub-gens is styled "u'kigdha'sne" (from uga'sne, to split a log), and denotes "One of the divisions of the common stock."

This exhibit of the Omaha Gentile System is far from being exhaustive.

At each review of the subject new ideas have been suggested, the correctness or incorrectness of which, I hope, may be revealed by future investigations.

Mr. J. W. Powell remarked on this paper as follows:

The great interest in Mr. Dorsey's discussion of his subject at the present time arises from the condition of discussions concerning barbarism. Government has been supposed to have had its origin in patriarchy, but this view is apparently overthrown by the arguments and discussions of Mr. Lewis H. Morgan, upon kinship which he holds to be the foundation of social organization, and therefore of government.

Among the North American Indians we find not indeed the lowest known form of human society but certainly a very primitive one, which exemplifies one of the earliest stages in the progress of man from barbarism to civilization, and from the researches of ethnologists in other regions, the general features of this stage of development appear to have been characteristic of all portions of the human race. Mr. Dorsey has given an account of the Dakota nations from which it appears that the gentes originated from the sub-division of older gentes. Among the Greeks and Romans two or more very nearly equal bodies constituted the curia or phratry. This imagined integration of smaller units was supposed to have arisen from the recognition of the necessity of some larger unit than the gentes. But among the Iroquois and Dakotas, the phratry is older than the gens. Here the process is rather one of differentiation. In also appears that the Indian social organization is not by any means simple, but is very elaborately organized.

In the case of the Wyandottes, the head of the household is a woman, and the line of descent is through the female. In the Omahas it is through the male. The Wyandotte gens has at its head four women who choose a male chief. There are eleven gentes,

and the council of the tribe is composed of four women and one male chief from each gens, and this council selects a sachem of the tribe. The herald of the tribe is the chief of the Wolf clan.

One of the functions of the gentile council is to select names for the young members of the clan. A list of names is made up, each being founded upon mythical stories of the bear, wolf, or other totemic animal, and once a year, at each green corn feast, the council meets to make up the list from which the names of children are selected.

Crime among the Wyandottes is divided into several classes of which the most important are theft, maiming, murder, and adultery. The mother punishes the daughter for fornication, or the four women counsellors condemn the culprit to whipping, or to have the hair shaved. Adultery is not punished by the husband, since the wife is not a member of his clan, but the punishment is inflicted by members of her own clan, and consists in cutting off the hair or the ears, and in disinheritance. Theft must be followed by restoration and compensation. Murder is sometimes compounded and sometimes is punished with death by strangling, or by the tomahawk before the council of the tribe. The herald of the Wolf clan selects one man from each gens, whose duty it is to perform the execution. Witchcraft is also a crime, and the Indian notions concerning it, bear a striking analogy to those of former generations of our own race. The accused person has the right of appeal which is salutory in its nature, consisting of wager or appeal to the Gods. The Wyandotte appeals to fire, and runs through it first from north to south, and then from east to west, and if he is burned he is guilty.

In the North American Indians the tribal organization had its origin in government by kinship. In no case does it originate in patriarchy. The lowest form of organization hitherto discovered is not found among them. This lowest form is where a body of men marry a body of women, as among the primitive Australians, and there are indications that this form of marriage once prevailed among the North American Indians. Probably the object of it was for purposes of defence.

Mr. W. B. Taylor suggested that the abstinence of certain gentes from eating certain animals or parts of animals, may have had a bearing upon the development of moral sentiments. Wherever we find man, we find "*taboo.*" This may perhaps spring from

a natural tendency to self-repression, and may lead to a factitious self-repression.

Mr. POWELL held the reverse view.

The next communication was by Mr. O. T. MASON, entitled

COMPARISON OF WRITTEN LANGUAGE WITH THAT WHICH IS SPOKEN ONLY.

In undertaking the study of any of our aboriginal languages, one is at once arrested by the difficulty of committing it to writing. Throughout his investigation he must constantly bear in mind the fact that the people who used the language had no graphic method of representing it. This consideration led the writer to inquire into the real differences that exist between written language and that which is not yet sufficiently developed to enter the written stage.

Spoken language is a compromise between the speaker and the hearer. The former is the creator of language; he is also its destroyer. The latter is its preserver. Men may speak as they please, indeed, they are continually making changes, such as coining terms, eliding portions of words, etc. This alteration of speech would go on indefinitely, were it not that the prime object of speaking, the desire to be understood, brings the speaker under obligation to the hearer.

Thus spoken language obeys two forces, a centrifugal and a centripetal, and its condition at any time is the resultant of these two.

On the other hand, written language is the product of four agencies—the speaker, the hearer, the writer, and the reader. Spoken language is addressed to the ear, and obeys the laws of audition. Written language is addressed to the eye, and is amenable to the laws of vision. It will readily occur to one engaged in the study of language, how much more intelligence it would require to manage so complex a machine, how liable some of the parts are to get out of order, and how the constant effort to readjust disturbances would itself elevate the people using a written language.

This difference manifests itself in the form of language. The classification of languages into polysynthetic, agglutinated, inflected, and monosyllabic, is subject to a higher generalization, namely, into the unwritten and the written. A people who never

see their language must necessarily encapsulate their sentences. But as soon as any one of our Indian languages is reduced to writing, and the tribe learns to read the printed book, the agglutination begins to vanish, the elements of the holophrasm segregate, and a copy of an Indian Bible presents a similar appearance to an ordinary English book, certainly bearing not a larger proportion of long words than one of Lord Macaulay's Essays.

Again, written language differs from spoken in its efficiency as an instrument of thought. As is well known, the roots or fundamental forms of language are few in number. The variety of ideas and thoughts which are in use among a people must be attained by such devices as the moves in chess, or the combinations of figures and letters in algebra. These devices are location in the sentence, collocation in the sentential term, affixes or figures of addition, figures of subtraction, figures of substitution, metathesis, intonation and accent, and many others.

As long as a people are in savagery or barbarism, the combinations necessary to supply their wants are within the resources of memory. There comes a time, however, when advancement must cease unless memory can be subsidized. Writing, therefore, marks an important era in the history of culture. It is the product, the receptacle, and the instrument of thought—just as a vase is the product of the art of pottery, a receptacle in the art of husbandry, and an instrument in the art of cookery.

Summing up the differences between languages that are written and those that are spoken only, the conclusion was reached that in the evolution of civilization the invention of writing marks as important an epoch as the acquisition of speech itself.

The Society then adjourned.

183D MEETING. JUNE 5TH, 1880.

The President in the Chair.

Twenty-eight members present.

The first communication for the evening was by Mr. PETER COLLIER on

SUGAR FROM SORGHUM.

Mr. Collier prefaced his remarks by mention of the earlier attempts to obtain sugar from the sorghum plant. Although molasses

and syrup had been frequently produced, yet the manufacture of crystallized sugar had not on the whole been attended with much success. Several years ago, Mr. J. Stanton Gould, of New York, had suggested that a few systematic and careful experiments ought to be made, but so far as known, none had been undertaken until recently. In 1879, the Agricultural Department resolved to experiment in this direction, and obtained seed for four varieties, the Early Amber, the White Liberian, the Chinese and the Honduras. These were planted May 15th, and a series of experiments was made beginning soon after the flower appeared and repeated every four or five days, until frost appeared in October.

Full details in regard to these experiments will be given in the annual report for 1879 of the Commissioner of Agriculture. The following are the more important results:

July 18th. The juice expressed from the Early Amber contained four per cent. of crystallizable and 3½ per cent. of uncrystallizable sugar.

August 15th. The juice contained 14.75 per cent. crystallizable, and 1½ per cent. uncrystallizable sugar. The latter proportions were maintained until heavy frosts in October.

The same results approximately were shown by the White Liberian upon the same dates.

August 6th. The Chinese variety showed less than two per cent. of crystallizable, and about five per cent. of uncrystallizable sugar; but with the progress of the season, the former gradually increased and the latter decreased.

The Honduras variety behaved in a manner very similar to the Chinese, but developed still later, reaching a maximum on the 18th of October. The general results of these experiments seemed to show that the failure hitherto to obtain sugar from sorghum was due to the custom of cutting the canes too early, and before they were ripe. If they had been permitted to stand as long as possible, before the inception of change by the action of frost, the amount of sucrose would have been much greater and the glucose much less.

The second communication was by Mr. WILLIAM McMURTRIE,

ON THE METEOROLOGICAL CONDITIONS AFFECTING THE CULTURE OF THE SUGAR BEET.

Mr. McMURTRIE's paper is to be published in full in the annual report for 1879 of the Commissioner of Agriculture.

184TH MEETING. JUNE 19TH, 1880.

The President in the Chair.

Twenty-six members present.

The President announced to the Society the election as members of Messrs. C. H. DAVIS, Z. L. WHITE, A. W. GREELY, C. E. KILBOURNE and J. P. STORY.

The first communication for the evening was by Mr. D. P. TODD

ON A MECHANICAL ATTACHMENT FOR EQUATORIAL MOUNTINGS TO FACILITATE SWEEPING IN RIGHT ASCENSION.

The arc of the sector which drives the polar axis is graduated in hours and parts thereof. Sliding upon this graduation are two vernier-like pieces, each of which carries a projecting metallic point. By clamp-screws the verniers may be attached to any part of the sector-arc. A revolving collar, with a screw for clamping, surrounds the polar axis adjacent to the sector, and carries a projecting arm the end of which will just touch both the metallic points attached to the verniers. An electric apparatus is so disposed that whenever the projecting arm comes in contact with either point on the verniers, a telegraphic sounder shall beat, or an electric bell shall ring. In using the device, the two verniers are set, by means of the sector-graduation, at a distance apart equal to the length of the zones to be searched. The sector is unclamped from the polar axis, and connected with the clock, and the latter set in motion. The telescope is then set upon one end of the zones, and the projecting arm brought in contact with the corresponding vernier-point. The collar is clamped to the polar axis in that position, and the telescope set in declination. As the instrument is moved back and forth in right ascension, the electric signal will apprise the ear of the observer whenever the telescope reaches either limit of the zones.

Mr. Todd's paper is published in full in the *Proceedings of the American Academy of Arts and Sciences*, 1880, page 270; also in the *Monthly Notices of the Royal Astronomical Society* for June, 1880.

The second paper was by Mr. THOMAS CRAIG on

VORTEX MOTION IN ORDINARY FLUIDS.

This paper is to be published in full in the Proceedings of the London Mathematical Society.

The third communication was by Mr. EDGAR FRISBY

ON MAGIC SQUARES.

[Abstract.]

The ordinary idea of a magic square is a square divided up into a smaller number of squares, with the successive integers placed in each square in such a way that the sum of the numbers, whether counted horizontally, diagonally, or vertically, will always be the same—many very ingenious such squares have been constructed, also squares of squares, magic cubes, and magic circles. I propose to simply show what I consider the most convenient way of constructing them when they can be constructed, and pointing out some of their remarkable properties.

The two which follow are remarkable illustrations of what I will call unsymmetrical squares:

Fig. I.

10	16	5	3
1	7	14	12
15	9	4	6
8	2	11	13

Fig. 2.

16	14	8	2	25
3	22	20	11	9
15	6	4	23	17
24	18	12	10	1
7	5	21	19	13

The first square counts 34 the ordinary 10 ways, each small square of 4 at the corners also counts 34, or the whole square can be divided into 4 squares each of which counts 34, and the 4

numbers at the angular points of the square as well as the middle squares all count the same number 34. There are also in the figures many more parallelograms whose sum is equal to 34.

In the second figure there are 12 ways in which the 5 numbers sum up to 65, but beyond this fact there appears to be no special symmetrical arrangement of the numbers.

The following figures appear to fulfil the conditions of symmetry.

Fig. 3.

1	15	14	4
12	6	7	9
8	10	11	5
13	3	2	16

Fig. 4.

11	24	7	20	3
4	12	25	8	16
17	5	13	21	9
10	18	1	14	22
23	6	19	2	15

The special characteristic of these two figures is, that any two figures whatever similarly placed with reference to the centre will always amount to the same number, in the first to 17, and in the second figure to 26; hence, in the first figure there will be 8 separate lines which add up to 17; this of course will only be by considering 6 and 11 as a different line to 1 and 16, and the same for the other diagonal. If now any one of these lines is considered as the diagonal of a parallelogram, and any one of the remaining lines the other diagonal, we shall have $\frac{8 \times 7}{2} = 28$ parallelograms similarly situated with respect to the centre, the sum of the numbers at the angular points being equal to 34; it will be observed that these parallelograms include the limiting case when the diagonals coincide with the two straight lines 1, 6, 11, 16, and 4, 7, 10, 13; they also of course include the central square and the 4 angular points of the whole square, besides these 28 there are the 4 squares into which the squares may be divided also the 4 horizontal and 4 vertical lines and 8 more rectangles, viz: 1, 14, 7, 12; 15, 6, 4, 9, and 6 others from the other sides, making altogether 48 straight

lines or parallelograms, for which the sum is equal to 34, and the sum of two figures on the straight line equidistant from the center =17.

Figure 4, which has the 5 squares on a side, will readily be perceived to be symmetrical. The sum of two symmetrical numbers always being equal to 26, we have of course a great number of parallelograms, for which the sum of the numbers at the angular points is always equal to 52, and a number of hexagons having the sum of the numbers at the angular points equal to 78, all which have this property that the diagonals joining opposite joints will always be mutually bisected at the centre, &c.

The magic square of six on a side cannot be constructed, at least not symmetrical like these others, this may be shown as follows:

Fig. 5.

1	a	b	c	d	
e	8	f	g		h
k	l	15		m	m
n′	m′		22	l′	k′
h′		g′	f′	29	e′
	d′	c′	b′	a′	36

If in figure 5 we attempt to fill up the diagonals so that the sum of the extremes shall be equal to 37, their difference must be an odd multiple of 5; the only numbers that can fill these conditions are:

1	8	15	22	29	36
6	11	16	21	26	31
11	14	17	20	23	26
16	17	18	19	20	21

The 2d and 3d, 2d and 4th, and 3d and 4th cannot co-exist,

therefore the 1st line must be one of the diagonals, and one of the other three lines the other diagonal.

Making $a + a' = b + b'$ &c. $= 37$, and putting in the conditions for a magic square we have:

$$\begin{aligned} a+b+c+d &= 104,\ 99 \text{ or } 94 \\ e+f+g+h &= 92,\ 89 \text{ or } 86 \\ k+l+m+n &= 80,\ 79 \text{ or } 78 \\ e-h+k-n &= 5,\ 10 \text{ or } 15 \\ a-d+l-m &= 3,\ 6 \text{ or } 9 \\ b-c+f-g &= 1,\ 2 \text{ or } 3 \end{aligned}$$

Whence, by addition, $2\,(a+b+e+f+k+l) = 285$, an odd number, which is impossible.

If we had constructed the square having 4 squares on each side in a similar way, we should have obtained two solutions, but in one of the solutions one of the numbers would have been used twice, and another one not at all, and so, for that reason it would have been excluded, therefore the one given is the only symmetrical solution; but there will be eight ways of arranging it, *viz:* by beginning at each corner and filling to the right or left.

The construction is very simple, for of the eight ways of doing it we will commence one *below* the middle square and move diagonally to the *right*, taking care when we get to the bottom to suppose the whole square moved down and begin again at top; when we go over to the left, suppose the whole square moved over, &c., beginning again at the right, and, when one square is filled up, move from there diagonally to the left, or, what amounts to the same thing, move down two vertically from your last number, the square will be entirely symmetrical; the sum of any two figures similarly placed with reference to the centre will be 122. There are many more interesting features connected with it that I leave to the reader's ingenuity to suggest.

A square having an odd number of squares on each side can always be constructed very easily, thus taking 11 on a side as an example:

Fig. 6.

56	117	46	107	86	97	26	87	16	77	6
7	57	118	47	108	87	98	27	88	17	67
68	8	58	119	48	109	88	99	28	78	18
19	69	9	59	120	49	110	89	89	29	79
80	20	70	10	60	121	50	100	40	90	80
81	81	21	71	11	61	111	51	101	41	91
92	82	82	22	72	1	62	112	52	102	42
48	98	88	88	12	78	2	68	118	58	108
104	44	94	23	84	18	74	8	64	114	54
55	105	84	95	24	85	14	75	4	65	115
116	45	106	85	96	25	86	15	76	5	66

At the conclusion of Mr. FRISBY's remarks the President announced that, by resolution of the General Committee, the Society stood adjourned until the second Saturday in October.

ADDENDUM.

On page 120, line 2, add the following paragraph:

The phenonema seem to be independent of the material of which the jaws of the slit are composed, for the same results followed in the case of brass, steel, and obsidian. In the case of the slit made of obsidian, the effects were a little more marked than with the metals, which may be attributed to the greater accuracy of the edges.

STANDING RULES.

At the meeting of the General Committee, June 19, 1880, it was—

Resolved, That the Secretary of the General Committee keep a chronological register of the elections and acceptances of Members of the Philosophical Society of Washington, showing the date at which each newly elected member accepts his membership; and that no person be admitted to the privileges of membership until his name is entered thereon as having accepted the same in writing.

Vide Standing Rules for the Society, No. 9, and Standing Rules for the General Committee, No. 7.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ELECTED NOVEMBER 8, 1879.

President, ... SIMON NEWCOMB.

Vice-Presidents, ... J. K. BARNES, W. B. TAYLOR, J. E. HILGARD, J. C. WELLING.

Treasurer, ... CLEVELAND ABBE.

Secretaries, ... C. E. DUTTON,* T. N. GILL.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

THOMAS ANTISELL, §
J. R. EASTMAN. †
E. B. ELLIOTT,
W. HARKNESS,
GARRICK MALLERY,
J. W. POWELL,
C. A. SCHOTT,
J. M. TONER,
J. J. WOODWARD.

STANDING COMMITTEES.

On Communications, ... C. E. DUTTON, CLEVELAND ABBE. ‡

On Publications .... S. F. BAIRD, ‖ T. N. GILL, C. E. DUTTON, CLEVELAND ABBE.

* *Vice* E. S. HOLDEN, resigned. † *Vice* C. E. DUTTON.

‡ *Vice* J. J. WOODWARD, resigned. § *Vice* W. H. DALL, resigned.

‖ As Secretary of the Smithsonian Institution.

LIST OF MEMBERS

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON,

Corrected to July 20th, 1880.

The names of the Founders of the Society, March 18, 1871, are printed in small capitals; for other members the dates of election are given.

§ indicates a life member by payment of 100 dollars.

* indicates absent from the District of Columbia, and excused from dues until announcing their return.

** indicates resigned.

? indicates dropped for non-payment, or nothing known of him.

† indicates deceased.

N. B.—It is scarcely possible for the Treasurer to keep a correct record of those who are absent and excused from paying dues, unless members will keep him duly notified of their removals.

Thomas Antisell.		
Cleveland Abbe	1871,	October 29.
Benjamin Alvord	1872,	March 28.
Asa O. Aldis	1873,	March 1.
Sylvanus Thayer Abert	1875,	January 30.
Robert Stanton Avery	1879,	October 11.
Spencer Fullerton Baird.		
Joseph K. Barnes.		
Stephen Vincent Benét.		
John Shaw Billings.		
Orville Elias Babcock	1871,	June 9.
Henry Hobart Bates	1871,	November 4.
† Theodorus Bailey	1873,	March 1.
Thomas W. Bartley	1873,	March 29.
Samuel Clagett Busey	1874,	January 17.
Emil Bessels	1875,	January 16.
George Bancroft	1875,	January 30.
* Lester A. Beardslee	1875,	February 27.
Rogers Birnie	1876,	March 11.
Marcus Baker	1876,	December 2.
Swan Moses Burnett	1879,	March 29.
Alexander Graham Bell	1879,	March 29.
William Birney	1879,	March 29.
Horatio Chapin Burchard	1879,	May 10.

HORACE CAPRON.
THOMAS LINCOLN CASEY.
†SALMON PORTLAND CHASE.
JOHN HUNTINGTON CRANE COFFIN.
†BENJAMIN FANEUIL CRAIG.
CHARLES HENRY CRANE.
Richard Dominicus Cutts .... 1871, April 29.
* Augustus L. Case .... 1872, November 16.
Robert Craig .... 1873, January 4.
Elliott Coues .... 1874, January 17.
Josiah Curtis .... 1874, March 28.
John White Chickering .... 1874, April 11.
* Frank Wigglesworth Clarke .... 1874, April 11.
Edward Clark .... 1877, February 24.
Frederick Collins .... 1879, October 21.
Thomas Craig .... 1879, November 22.
John Henry Comstock .... 1880, February 14.

WILLIAM HEALEY DALL.
†ALEXANDER B. DYER.
Clarence Edward Dutton .... 1872, January 27.
† Richard Crain Dean .... 1872, April 28.
Henry Harrison Chase Dunwoody .... 1873, December 20.
† Charles Henry Davis .... 1874, January 17.
† Frederic William Dorr .... 1874, January 17.
Myrick Hascall Doolittle .... 1876, February 12.
** George Dewey .... 1879, February 15.
Charles Henry Davis .... 1880, June 19.

† AMOS BEEBE EATON.
EZEKIEL BROWN ELLIOTT.
** GEORGE H. ELLIOT.
John Robie Eastman .... 1871, May 27.
* Stewart Eldredge .... 1871, June 9.
? Fredric Miller Endlich .... 1873, March 1.
? Charles Ewing .... 1874, January 17.
* Hugh Ewing .... 1874, January 17.
John Eaton .... 1874, May 8.

* ELISHA FOOTE.
William Ferrel .... 1872, November 16.
Edgar Frisby .... 1872, November 16.
† John Gray Foster .... 1873, January 18.
Edward T. Fristoe .... 1873, March 29.
Robert Fletcher .... 1873, April 10.
Edward Jessop Farquhar .... 1876, February 12.

THEODORE NICHOLAS GILL.
BENJAMIN FRANKLIN GREENE.
Henry Goodfellow .... 1871, November 4.
Grove Karl Gilbert .... 1873, June 7.
Leonard Dunnell Gale .... 1874, January 17.
* James Terry Gardner .... 1874, January 17.
George Brown Goode .... 1874, January 31.
Henry Gannett .... 1874, April 11.
* Edward Oziel Graves .... 1874, April 11.
Edward Miner Gallaudet .... 1875, February 27.
Francis Vinton Greene .... 1875, April 10.
Francis Mathews Green .... 1875, November 9.

Edward Goodfellow — 1875, December 18.
Alexander Young P. Garnett — 1878, March 16.
* Walter Hayden Graves — 1878, May 25.
* Francis Mackall Gunnell — 1879, February 1.
Bernard Richardson Green — 1879, February 15.
William Whiting Godding — 1879, March 29.
James Howard Gore — 1880, March 14.
Adolphus W. Greely — 1880, June 19.

ASAPH HALL.
WILLIAM HARKNESS.
FERDINAND VANDEVEER HAYDEN.
† JOSEPH HENRY.
JULIUS ERASMUS HILGARD.
ANDREW ATKINSON HUMPHREYS.
Henry W. Howgate — 1873, January 18.
Edward Singleton Holden — 1873, June 21.
† Isaiah Hanscom — 1873, December 20.
* Edwin Eugene Howell — 1874, January 31.
Henry Wetherbee Henshaw — 1874, April 11.
David Lowe Huntingdon — 1877, December 21.
George William Hill — 1879, February 1.
* Peter Conover Hains — 1879, February 15.
* Franklin Benjamin Hough — 1879, March 29.
William Henry Holmes — 1879, March 29.
Ferdinand H. Hassler — 1880, May 8.

THORNTON ALEXANDER JENKINS.
William Waring Johnston — 1873, June 21.
* Henry Arundel Lambe Jackson — 1875, January 30.
William Nicholson Jeffers — 1877, February 24.
Arnold Burgess Johnson — 1878, January 19.
Joseph Taber Johnson — 1879, March 29.
Owen James — 1880, January 3.

* Reuel Keith — 1871, October 29.
John Jay Knox — 1874, May 8.
Albert Freeman Africanus King — 1875, January 16.
† Ferdinand Kampf — 1875, December 18.
Clarence King — 1879, May 10.
Jerome H. Kidder — 1880, May 8.
Charles Evans Kilbourne — 1880, June 19.

† JONATHAN HOMER LANE.
Nathan Smith Lincoln — 1871, May 27.
** Henry H. Lockwood — 1871, October 29.
** Stephen C. Lyford — 1873, January 18.
William Lee — 1874, January 17.
Edward Phelps Lull — 1875, December 4.
Eben Jenks Loomis — 1880, February 14.

† FIELDING BRADFORD MEEK.
MONTGOMERY CUNNINGHAM MEIGS.
ALBERT J. MYER.
William Myers — 1871, June 23.
† Oscar A. Mack — 1872, January 27.
William Manuel Mew — 1873, December 20.

† Archibald Robertson Marvine .... 1874, January 31.
† James William Milner .... 1874, January 31.
Garrick Mallery .... 1875, January 30.
Otis Tufton Mason .... 1875, January 30.
William McMurtrie .... 1876, February 26.
Aniceto Gabriel Menocal .... 1877, February 24.
Martin Ferdinand Morris .... 1877, February 24.
Montgomery Meigs .... 1877, March 24.
* Joseph Badger Marvin .... 1878, May 25.
Frederic Bauders McGuire .... 1879, February 15.
Clay Macauley .... 1880, January 3.

SIMON NEWCOMB.
WALTER LAMB NICHOLSON.
* Charles Henry Nichols .... 1872, May 4.
Charles Nordhoff .... 1879, May 10.

GEORGE ALEXANDER OTIS.
John Walter Osborne .... 1878, December 7.

JOHN GRUBB PARKE.
PETER PARKER.
* TITIAN RAMSAY PEALE.
* BENJAMIN PEIRCE.
* Charles Christopher Parry .... 1871, May 13.
** Carlisle P. Patterson .... 1871, November 17.
* Charles Sanders Peirce .... 1873, March 1.
Orlando Metcalfe Poe .... 1873, October 4.
John Wesley Powell .... 1874, January 17.
** David Dixon Porter .... 1874, April 11.
* Albert Charles Peale .... 1874, April 11.
Robert Lawrence Packard .... 1875, February 27.
Henry Martyn Paul .... 1877, May 19.
Henry Smith Pritchett .... 1879, March 29.
Daniel Webster Prentiss .... 1880, January 3.

* Christopher Raymond Perry Rodgers .... 1872, March 9.
* Joseph Addison Rogers .... 1872, March 9.
John Rodgers .... 1872, November 16.
* Henry Reed Rathbone .... 1874, January 17.
* Robert Ridgway .... 1874, January 31.
† John Campbell Riley .... 1877, May 19.
Charles Valentine Riley .... 1878, November 9.
William Francis McKnight Ritter .... 1879, October 21.

BENJAMIN FRANKLIN SANDS.
† GEORGE CHRISTIAN SCHAEFFER.
CHARLES ANTHONY SCHOTT.
WILLIAM TECUMSEH SHERMAN.
James Hamilton Saville .... 1871, April 29.
Ainsworth Rand Spofford .... 1872, January 27.
? Frederic Adolphus Sawyer .... 1873, October 4.
John Sherman .... 1874, January 17.
* John Stearns .... 1874, March 28.
* Ormond Stone .... 1874, March 28.
Aaron Nichols Skinner .... 1875, February 27.
Samuel Shellabarger .... 1875, April 10.

David Smith..........1876, December 2.
* Montgomery Sicard..........1877, February 24.
Henry Robinson Searle..........1877, December 21.
Charles Dwight Sigsbee..........1879, March 1.
John Patten Story..........1880, June 19.

WILLIAM BOWER TAYLOR.
* William Calvin Tilden..........1871, April 29.
? George Taylor..........1873, March 1.
Joseph Meredith Toner..........1873, June 7.
Almon Harris Thompson..........1875, April 10.
William J. Twining..........1878, November 23.
David P. Todd..........1878, November 23.

** Jacob Kendrick Upton..........1878, February 2.

George Vasey..........1875, June 5.

* JUNIUS B. WHEELER.
JOSEPH JANVIER WOODWARD.
William Maxwell Wood..........1871, December 2.
Francis Amasa Walker..........1872, January 27.
James Clarke Welling..........1872, November 16.
James Ormond Wilson..........1873, March 1.
George M. Wheeler..........1873, June 7.
† John Maynard Woodworth..........1874, January 31.
Allen D. Wilson..........1874, April 11.
? Charles Warren..........1874, May 8.
* Joseph Wood..........1875, January 16.
* Christopher Columbus Wolcott..........1875, February 27.
Lester Frank Ward..........1876, November 18.
Charles Abiathar White..........1876, December 16.
Zebulon L. White..........1880, June 19.

† Mordecai Yarnall..........1871, April 29.
Henry Crissey Yarrow..........1874, January 31.

Anton Zumbrock..........1875, January 30.

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The Editor of Archives Néerlandaises des Sciences, etc.

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Library of the K. Danske Videnskabernes Selskab.

INDEX TO NAMES OF CONTRIBUTORS

To Volume III.